DISCUSSION, PROCEEDINGS V. 56

Discussion of papers published in the October through December, 1959, JOURNALS appears in the concluding pages of this June issue, as the Institute continues its quarterly publication of discussion. Discussion of papers published January through March, 1960, will appear in Part 2, September 1960 ACI JOURNAL. Discussion of April through June, 1960, papers will appear in Part 2, December 1960 JOURNAL along with index and errata for V. 56.

Discussion of a paper by M. F. Kaplan:

Flexural and Compressive Strength of Concrete as Affected by the Properties of Coarse Aggregates*

Discussions by J. F. McLaughlin, B. G. Singh, and Stanton Walker and Delmar L. Bloem appeared in ACI JOURNAL, Part 2, Dec. 1959 (Proceedings V. 55), pp. 1605-1616.

AUTHOR'S CLOSURE

The interest in the paper shown by Messrs. McLaughlin, Singh, Walker and Bloem is very much appreciated.

Professor McLaughlin is of the opinion that an analysis of variance should have been done to ascertain whether the different aggregates did in fact cause significant variations in concrete strength. Coarse aggregates with entirely different properties may, however, not cause significant differences in concrete strength — e. g., Aggregate A, with a high roughness factor and low angularity number, may result in concrete having approximately the same strength as Aggregate B, which has a low roughness factor and high angularity number. This may be due to the compensating effects of the different properties and does not mean that the surface texture and shape of the aggregates do not themselves affect concrete strength. The object of the investigation was to ascertain which *specific* properties of coarse aggregates may be related to the strength of concrete and not whether coarse aggregates *per se* cause differences in strength. For these reasons the analysis of variance, suggested by Professor McLaughlin, is not considered to be necessary. The multiple regression analyses were performed, using mean values for each aggregate, for each of the concrete mixes tested at different ages. Th average variance in flexural strength about the regression equation was 16 percent of the over-all variance. This value did not vary greatly in the individual regression analyses for different mixes and ages and it is considered that little error is introduced by quoting the average result.

Dr. Singh agrees that Eq. (1) and (2) indicate that strength was mainly influenced by the modulus of elasticity, angularity, and surface texture of the coarse aggregates used in the investigation. He has, however, compared observed strengths at different ages with strengths calculated from Eq. (1) and (2). Because the correlation was poor he

*ACI JOURNAL, V. 30, No. 11, May 1959 (*Proceedings* V. 55), p. 1193. Disc. 55-72 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 31, No. 12, June 1960 (*Proceedings* V. 56).

suggests that there are other factors which affect strength. Eq. (1) and (2) are, however, the average equations for all mixes at all ages. The individual regression equations for each mix and age were calculated. Comparison of the observed strengths, with strengths calculated from the appropriate equation, indicated that the scatter about the line of equality was approximately the same as that shown in Fig. 5 and 6. Dr. Singh's comments on the possible effects of differences in surface texture, modulus of elasticity, and angularity of the coarse aggregate are interesting. He suggests that an increase in surface area, due to greater angularity, will lead to a decrease in cement per unit aggregate area and so cause a reduction in strength. It has, however, been shown that for the same water-cement ratio, an increase in aggregate-cement ratio, i.e., a decrease in cement per unit aggregate area, results in an *increase* in strength.[2*] If the strength producing properties of an aggregate can be represented by a single parameter ϕ, as stated by Dr. Singh, it would certainly be an advantage in calculating mixes. It may be noted that the regression equations, given in the paper, are linear in nature thus making it possible to reduce the three parameters, e, a, and s to a single one. As mentioned above, the main object of the investigation was, however, to throw some light on a more fundamental understanding of the effects of *specific* characteristics of aggregates.

The mix proportions, by weight, were intentionally kept constant. As Messrs. Walker and Bloem state this resulted in concretes of different workability. This has been fully discussed elsewhere[15] by the author and did in fact constitute one part of the investigation into the effects of the properties of coarse aggregates on the properties of concrete. It is, however, not clear to the author why differences in workability or consistency are considered to cause differences in the strength of concretes which have the same water-cement ratio and which have been fully consolidated. The same applies to differences in the ratio of fine to coarse aggregate.

As mentioned above, different properties of the same aggregate may have different effects on concrete strength. Thus aggregates with entirely different properties may produce concrete of approximately the same strength because of compensating effects. Messrs. Walker and Bloem's statement that variations in strength were not enough to leave much room for influence by any of the variables is therefore not thought to be appropriate.

Because of the simple correlation which was found between the modulus of elasticity and specific gravity of the aggregates it is contended that the relationship between elasticity of the aggregate and the strength of concrete may be indirectly due to cement content. It has, however, been found[2] that for the same water-cement ratio an increase in the

*See references ACI JOURNAL, V. 30, No. 11, May 1959 (*Proceedings* V. 55), pp. 1207-1208.

cement-aggregate ratio leads to a *decrease* in concrete strength, whereas in Fig. F, Messrs. Walker and Bloem indicate that an increase in the specific gravity of the aggregate, i.e., an increase in cement factor and therefore in the cement-aggregate ratio, is accompanied by an *increase* in concrete strength. In the opinion of the author the increase, instead of decrease, in strength may be due to the effects of the other properties of the aggregate which override the effects of the elasticity of the aggregate. It is for this reason that simple correlation coefficients are not satisfactory in an analysis of this kind and it is therefore unlikely that the effects of modulus of elasticity of the aggregate may be due to the indirect effects of differences in specific gravity on the cement content of the mix. The variations in the cement content were in fact very small and are not considered to have had a large effect on concrete strength. Differences in the angularity of the coarse aggregate undoubtedly caused differences in workability and in excess sand content, but, as mentioned above, it is not clear why for the same water-cement ratio and full consolidation, these should be considered to affect concrete strength and therefore indirectly account for the effects of angularity.

It should, nevertheless, be mentioned that the existence of a statistical relationship between the modulus of elasticity, angularity, and surface texture of the aggregates and concrete strength, does not necessarily establish that these properties *cause* differences in strength. It is, however, hoped that the investigation may serve, as mentioned by Dr. Singh, as a foundation for future investigations into the strength of concrete.

Discussion of a paper by David Watstein and Robert G. Mathey:

Width of Cracks in Concrete at the Surface of Reinforcing Steel Evaluated by Means of Tensile Bond Specimens*

By VOLKER HAHN and AUTHORS

Discussion by Bruce H. Falconer appeared in March 1960 ACI JOURNAL, p. 929.

By VOLKER HAHN†

Messrs. Watstein and Mathey carried out measurements on tensile bodies of a relatively large concrete section to determine the width of cracks near the steel.

In 1935 F. von Emperger published in *Mitteilungen des Österreichischen Eisenbetonausschusses,* V. 16, an article dealing with the extension of concrete at various distances from the steel. He found that with a concrete of low strength (W_b = 123 kg per sq cm) the extension will be negligible when using a smooth bar; the extension will go up to 40 percent of the elongation of steel when using deformed bars (so-called *Drillwulststahl*). With a concrete of higher strength (W_b = 330 kg per sq cm) Emperger states that with both smooth or deformed bars, the latter having only a low bond strength compared with our modern ribbed steel bars, no extension was noticeable.

In 1952, at the Institute of Professor Graf, I studied the extension of concrete on steel with a variety of ribbed steel bars. The test installation is shown in Fig. A. Two rings of different diameters were put over the steel. The inner ring was fastened to the steel while the outer ring was supported by the concrete. Thus the extension of the concrete could be measured directly on the steel. The concrete strength as determined on a 20-cm cube was 250 kg per sq cm.

The ribbed bars tested are shown in Fig. B. The extension of the steel and the concrete as a function of the steel tension are plotted in Fig. C. The difference between the extension of the steel and the

*ACI JOURNAL, V. 31, No. 1, July 1959 (*Proceedings* V. 56), p. 47. Disc. 56-7 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 31, No. 12, June 1960 (*Proceedings* V. 56).

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concrete is the actual denudation of the steel bars.

The results confirm the research work of Messrs. Watstein and Mathey. With smooth bars only a very small extension can be noticed. With ribbed steel bars the concrete only follows the initial movement of the steel to a small extent, while during the larger movements the actual pulling-out, that is the denudation of the steel, only shows a small increase. At $\sigma_e = 3000$ kg per sq cm the denudation only amounts

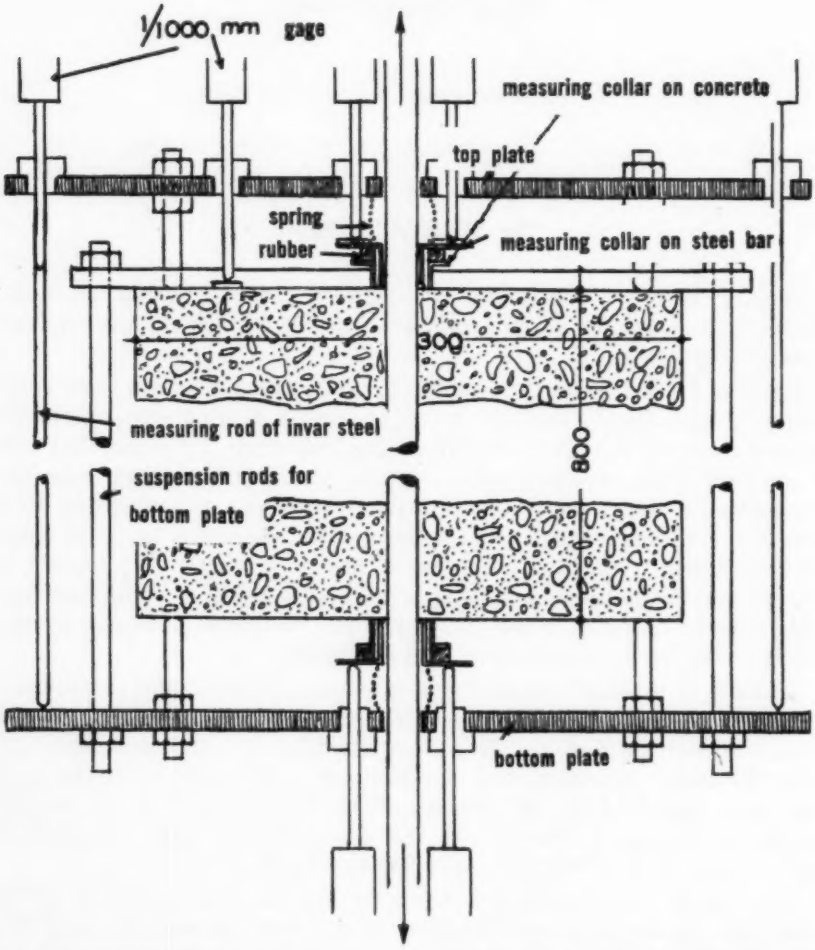


Fig A—Test installation. The movements at the point where the steel leaves the 30 x 30 x 80-cm concrete body are measured by means of two rings. The smaller ring is fastened to the steel, while the larger ring is supported by the concrete surface

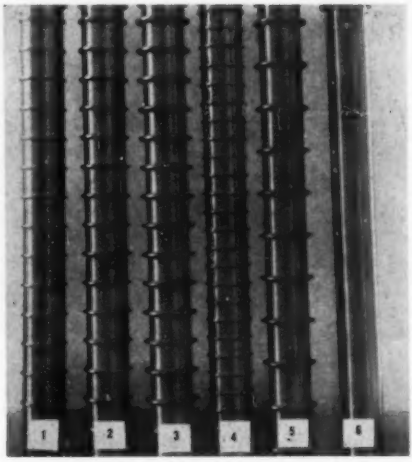


Fig. B—Round steel bars with various spacings of ribs that were used for the tests. Bar No. 6 is smooth

to 20-30 percent; that means the width of cracks close to the steel bars would only amount to 1/3 to 1/5 of the width of cracks on the concrete surface.

I think I found the reason for this in the sloping cracks that are shown in Fig. D. I called them secondary cracks as they always occur near the main crack. The development of these secondary cracks is explained in Fig. E. If the concrete is supposed to crack in the direction a-a, the main tensile stress must be

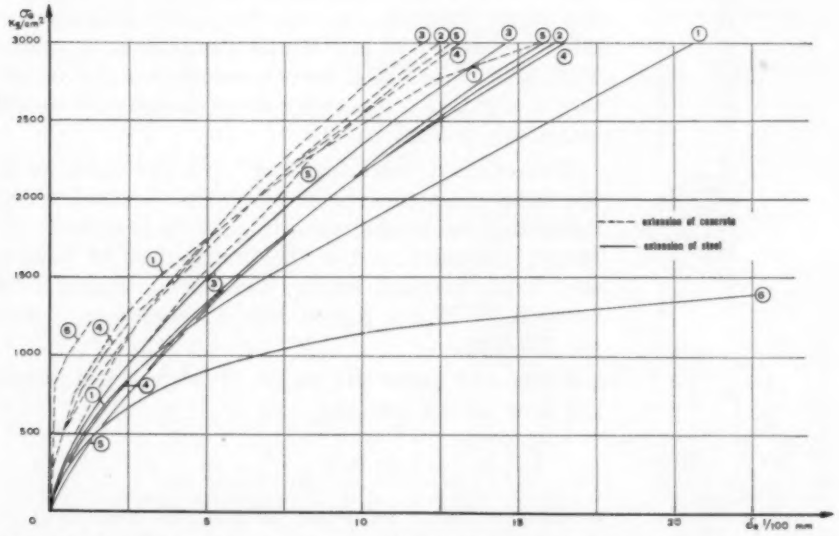


Fig. C—Extension of steel and concrete at the upper front surface of the tensile body. The bars shown in Fig. B were used

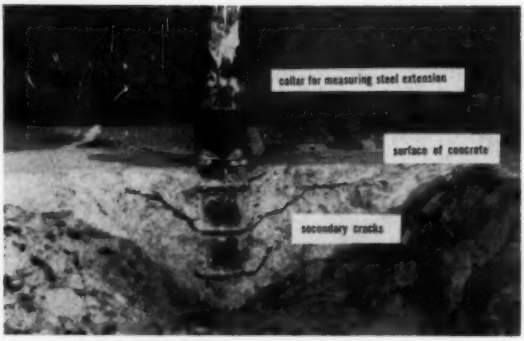


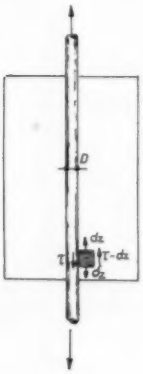
Fig. D—Secondary cracks at the point the bar enters the concrete

$$\sigma_z = \frac{P\sqrt{2}}{\pi\sqrt{2}\,(d_s^2 + d_s D)} > \beta_{bz}$$

β_{bz} representing the tensile strength of the concrete; or, as the force introduced on the circumference is $P = \pi D d_s \tau$:

$$\frac{\pi D d_s \tau \sqrt{2}}{\pi\sqrt{2}\,(d_s^2 + d_s D)} > \beta_{bz}$$

For the limit $d_s \to 0$, τ has to be $> \beta_{bz}$, if the sloping cracks are supposed to occur in the main tensile stress direction.



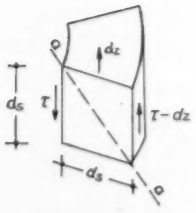


Fig. E

This explains the fact that with smooth steel bars (which means a small value of τ) extension of the concrete, i.e., development of secondary cracks, does not occur (except — as was found by Emperger — when a concrete of low tensile strength β_{bz} is used). With well ribbed steel bars, however, we always obtain $\tau \geqq \beta_{bz}$, i.e., the condition for occurrence of the secondary cracks is met.

This cracked "entering zone" and the decrease of the bond stress due to it was also observed when measuring the bond stress on steel bars [e.g., by R. M. Mains, "Measurement of the Distribution of Tensile and Bond Stresses Along Reinforcing Bars," ACI JOURNAL, V. 23, No. 3, Nov. 1951 (*Proceedings* V. 48), pp. 225-252].

I tried to express the length of this cracked entering zone in the formula:

$$x_e = \frac{\tau U}{\beta_{bz}\,\pi\sqrt{2}}\,\alpha$$

α being a factor between 0.6 and 2.0 according to the thickness of the concrete cover. The extension of the concrete increases with the steel bar diameter

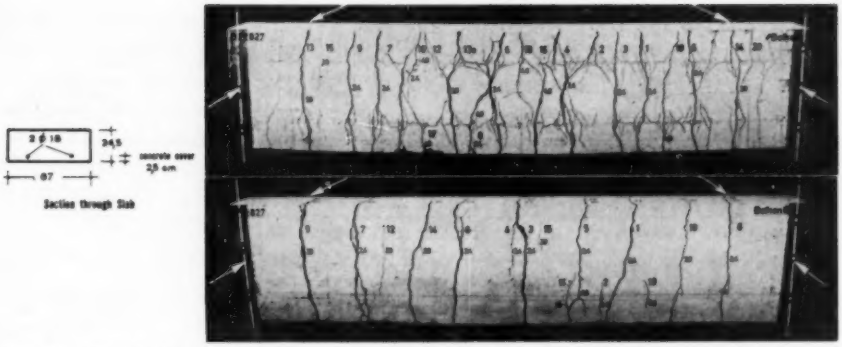


Fig. F—Crack formation of two concrete slabs that were reinforced with transverse-ribbed steel bars (top) and round steel bars (bottom)

and the bond strength and will be noticed to decrease at higher concrete tensile strength.

Can the results obtained from a big tensile body also be applied to a beam with a thin concrete cover? Also, does the width of cracks close to the steel only amount to a fraction of the visible width of crack on the concrete surface?

Fig. F shows the crack formation of a concrete slab reinforced with ribbed steel bars and another one reinforced with smooth bars. The development of cracks with ribbed steel is demonstrated in Fig. G. At $\sigma_e = 2000$ kg per sq cm two cracks of 0.05 mm width occurred. At

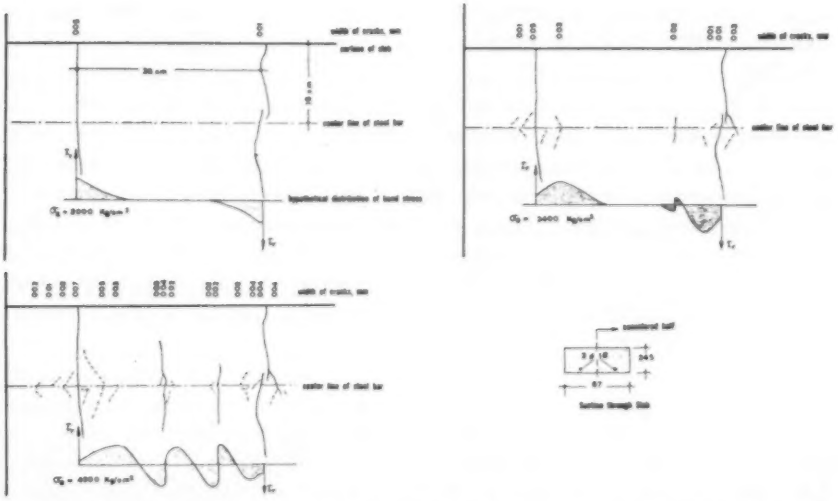


Fig. G—Development of cracks with ribbed steel bars. The plotted widths of cracks were measured in the axis of the bars

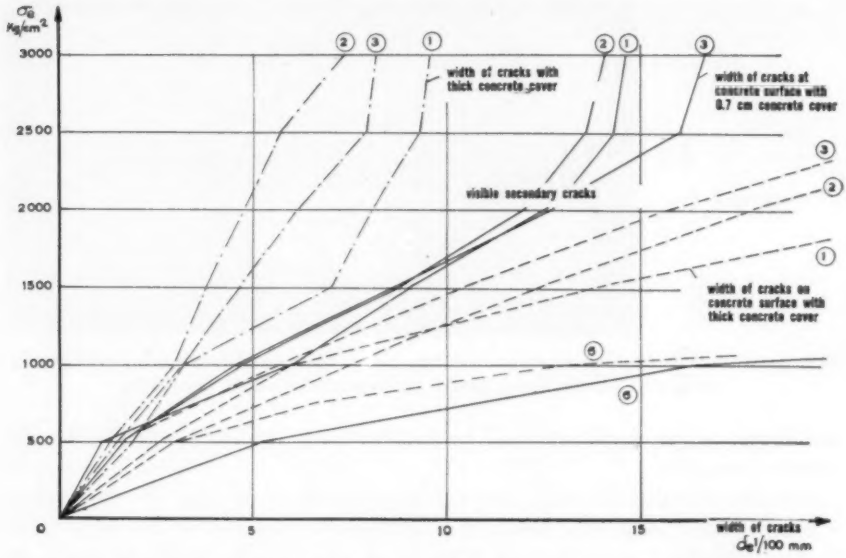


Fig. H—Widths of cracks with thick and thin concrete cover with ribbed steel bars of 18 mm diameter

$\sigma_e = 3000$ kg per sq cm, besides the main crack, secondary cracks can be noticed which are similar to those of Fig. D. At $\sigma_e = 4000$ kg per sq cm the left main crack only widened from 0.05 to 0.07 mm. The rest of the steel extension was taken up by the secondary cracks. With smooth steel bars (Fig. F) no secondary cracks can be observed.

In Fig. H the width of cracks is again shown for some of the steel bars of Fig. B. Here, with a thick concrete cover — as is the case with the tensile bodies studied by Messrs. Watstein and Mathey — the width of cracks on the concrete surface and on the steel can be noticed, the latter amounting to only a fraction of the width of crack noticeable on the concrete surface. These results were compared with the widths of cracks of a beam having a concrete cover of only 0.7 cm. It is clearly seen that after development of the secondary cracks, the main crack hardly widens. The measured widths of cracks approach the width of cracks on steel with thick concrete cover.

Regarding these findings, I got the opinion that the proportionality factors $W_s : W_c$ (width of cracks on steel : width of cracks on concrete surface) applied in Fig. 8 by Messrs. Watstein and Mathey cannot be transmitted to the beam with thin concrete cover. It can be presumed that the proportion $W_s : W_c$ is all the larger, the thinner the concrete cover is made. I suppose that by use of ribbed steel bars the width of cracks close to the steel has a similar absolute value regardless of a

thick or thin concrete cover although the widths of cracks on the concrete surface vary considerably.

Of course these considerations are only applicable for a concrete body with a small reinforcement ratio. At higher percentages of reinforcement the zone of the secondary cracks is smaller. In case of a very high percentage of reinforcement there will be no extension of concrete at all. In this case the width of cracks also will be very small. Thus the secondary cracks which can be observed on ribbed steel are—so to speak—a preventive, entering into action when larger cracks occur on the concrete surface.

Reference

4. Hahn, Volker, "Über die Verbundwirkung des Querrippenstahls," *Die Bauwirtschaft* (Wiesbaden), No. 3 and 4, Jan. 15 and 22, 1955, 11 pp.

AUTHORS' CLOSURE

Mr. Falconer computed from the data in our Fig. 6 that the bond stress in the ½-in. bar increased from 550 to 1200 psi as the steel stress increased from 20,000 to 90,000 psi; for a ⅞-in. bar the bond stress fell from 950 to 675 psi. The authors have no explanation for this curious reversal of the trend in stress variation. However, it is possible that the cosine distribution of bond stresses which Mr. Falconer had assumed may not be applicable at all levels of steel stress and different bond stress-steel stress relationships may result from different assumed distribution of bond.

The authors are aware of the fact that the observed maximum extension of concrete adjacent to the reinforcing bar would lead us to conclude that a tensile stress of about 2000 to 4000 psi is developed in the concrete. A tensile stress of such a magnitude, of course, cannot exist in concrete. We must assume, therefore, that the large observed extensions in concrete adjacent to the bar must be associated with the formation of microscopic cracks. Inasmuch as these microcracks originate in the confined region adjacent to the steel, it seems reasonable to assume that the cracks are restrained by the steel and are not permitted to extend to the free surface of the concrete. Mr. Falconer described graphically this state of concrete around the bar as ". . . a narrow annular concertina of cracked concrete around the bar and contained within the uncracked outer shell."

The authors are familiar with Emperger's work cited by Dr. Hahn. Emperger reported extension of low strength concrete ($f_c' = 1750$ psi) adjacent to the bar equal to 40 percent of the extension of the steel bar. The authors observed extension in concrete of about 70 percent of that in the steel with our modern deformed bars. Moreover, these extensions

were observed with concrete strengths of about 4200 psi, while Emperger reported no noticeable extension in concrete having a compressive strength of 4700 psi. As Dr. Hahn pointed out, Emperger's deformed bar was inferior to our present deformed bars and this may account for the fact that Emperger observed no noticeable extension in specimens of higher strength concrete.

The authors appreciate the thoughtful comments of Mr. Falconer and Dr. Hahn.

Discussion of a paper by G. C. Ernst and A. R. Riveland:

Ultimate Loads and Deflections from Limit Design of Continuous Structural Concrete*

By B. PRASANNA SINHA and AUTHORS

By B. PRASANNA SINHA†

There is one important item to which the authors have not called attention. The Uniqueness Theorem requires ultimate strength to be developed at enough cross sections to turn the structure into a mechanism. For developing ultimate strength those critical sections require an optimum rotation, without which critical strain will not be developed in the concrete. If these rotations are plotted against corresponding sections, they suggest an incompatible rotation diagram. In some cases they can be explained, but in many cases they present an awkward situation keeping in mind the moment-rotation diagram for concrete as presented by the authors in Fig. 2.

Take the case of a prismatic beam fixed at the ends to carry a uniformly distributed load. If d is the effective depth of the beam; $k_u d$, the depth of neutral axis for ultimate moment from the compression face, can be calculated by various formulas to be approximately equal to $1.30\ dpf_y/f_c'$ where p is the percentage of steel at the section and f_y and f_c' are yield and ultimate strength of steel and concrete, respectively. Assuming 0.003 as the critical strain in concrete (for instantaneous loading), the rotation Θ_u required for developing ultimate strength will be given by the following relationship:

$$\Theta_u = \frac{0.003\ f_c'}{1.30\ dpf_y} = \frac{0.0023\ f_c'}{dpf_y}$$

Now if the beam is designed in accordance with ACI 318-56, end sections will be assumed to carry a moment of $wL^2/12$ and the middle a moment of $wL^2/24$. Naturally, p in the middle will be approximately half of the p at either end and, other things remaining constant, the rotation in the middle section will be twice the rotation at any end.

As has already been used by the authors the $M/E_c I$ diagram will be proportional to the M diagram for the major portion of the beam and

*ACI JOURNAL, V. 31, No. 4, Oct. 1959 (*Proceedings* V. 56), p. 273. Disc. 56-19 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 31, No. 12, June 1960 (*Proceedings* V. 56).

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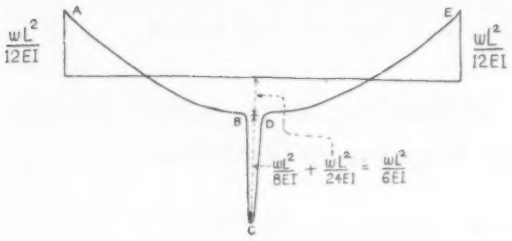

Fig. A

especially for the portion where the bending moment is well below in the elastic range. This results in a diagram as shown by ABCDE in Fig. A. BD cannot be expanded to a considerable distance because the area BCD has been carved out from the original negative area of the M/E_cI diagram by a negligible decrease in the ordinates at other sections; otherwise the negative area will become heavier than the positive, affecting the very geometry of a fixed beam.. In a way the height BC or DC is as good as plastic rotation, in this case equal to $(2 - 0.5) = 1.5$ times the rotation at A.

This suggests that design in accordance with ACI 318-56 is no guarantee against concentrated angle changes and a better set of moments might be tried with advantage; but those sets obtained from pure mathematical treatment without paying any attention to the actual rotation, may create trouble on the unsafe side.

As an example, it can be shown that with equal amounts of reinforcements provided as top and bottom steel at the end and middle sections, the same beam may be allowed to carry a moment equal to $wL^2/16$ at both the end and middle sections according to any of the three methods suggested by the authors. In this case, critical rotation will also be the same at these sections and hence the M/E_cI diagram should be as shown by ABCDE in Fig. B.

But in this diagram the negative area is heavier than the positive and the only possible solution could have been brought about by assuming a greater region at the ends under ultimate strength. But the ordinate cannot be higher than that at A, unless the load is applied with infinitely slow speed; and the region under ultimate strength cannot be expanded beyond B where the moment is well below in the elastic range. From the dimensions given in Fig. B it is evident that under the above conditions the positive area cannot be made equal to the negative.

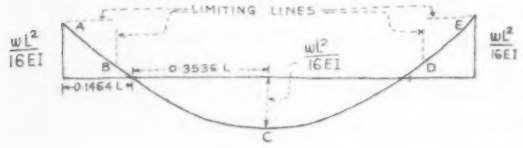

Fig. B

AUTHORS' CLOSURE

Mr. Sinha's discussion is most welcome in that it provides an opportunity to discuss certain misconceptions concerning the flexural properties of reinforced concrete at ultimate load. In general, it seems clear that the discussion is based upon a different viewpoint regarding the effective moment of inertia and the manner in which plastic rotation is developed at a plastic hinge. It may be that the authors have not stated their reasoning with sufficient clarity.

With regard to the effective moment of inertia, Fig. C presents $M/E_c I$ values calculated with the moments of inertia of the transformed cracked sections and the experimentally determined values of M_y, M_c, and M_{max} from Reference 2. There can be no doubt that the section was cracked for each of these moments inasmuch as the cracking was visually observed on a white washed surface and reported for these tests. As illustrated in Fig. C, the use of the transformed cracked section results in a value of $M/E_c I$ for yield, crushing, and maximum that rationally may be assumed constant. Application of this to a fixed end beam with uniformly distributed load will change Fig. A such that plastic hinging is required at A and E. This has been rather completely presented in various portions of References 4 and 5 and experimentally confirmed and discussed for a substantial range of concentrated loading conditions in Reference 3. Insofar as the effective moment of inertia is concerned throughout those portions of a beam below the ultimate strength, the various extremes of assumptions were presented and discussed in

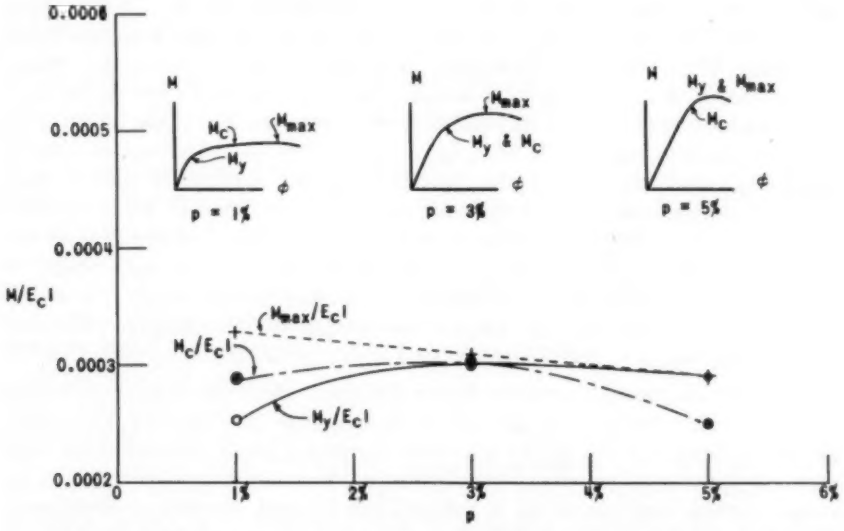

Fig. C—$M/E_c I$ versus p (each plotted point is average of five tests)

Reference 4 on pp. 629 and 630 and illustrated in Fig. 21 and 22 of that reference. It would have been most interesting if Mr. Sinha could have presented experimental confirmation of his convictions.

Concerning the plastic rotation developed at a plastic hinge, it must be clearly recognized that the total rotation at a plastic hinge of reinforced concrete is not only a function of the amount of plastic unit rotation at a critical section, but also of the spread of steel yield and high compressive concrete strain, local bond failure, cracking, and other local conditions such as bearing area and shape of knee or reentrant corner. This has been experimentally demonstrated and discussed in References 2 and 3 and also theoretically discussed in References 4 and 5. In addition to the references of the paper, plastic hinging in reinforced concrete has been theoretically presented and experimentally demonstrated for large rotations over short lengths under combined moment, thrust, and shear, for reinforced concrete articulations made at the National Bureau of Standards (ACI *Proceedings,* V. 31, 1935, p. 304), the University of Illinois (*Bulletin* No. 322, 1940), and the University of Maryland (ACI *Proceedings* V. 36, 1939, p. 49 and ASCE *Transactions,* V. 106, 1941, p. 862). In spite of Mr. Sinha's concern for simultaneous development of an optimum rotation and the ultimate strength at critical sections for application of the Uniqueness Theorem, ample total rotation capacity with ultimate strength has been repeatedly developed in tests by authors and others.

The authors agree with Mr. Sinha that a pure mathematical treatment for adjusting concentrated angle changes would be questionable, and upon careful study of the paper and its references he would recognize that a thorough foundation was laid on the basis of actual total rotational capacity. Mr. Sinha has apparently considered only the moment versus unit rotation relationship at a section, which does not provide a complete understanding (see pp. 630-632 of Reference 4). This has misled him into presenting Fig. B with his limiting lines as evidence of incompatibility, thereby completely overlooking the spread of plastic unit rotation as demonstrated in the tests of Reference 2 and 5. Furthermore, an inspection of Fig. 23 of Reference 4 will show that for the case of the fixed end beam with uniformly distributed load the spread needs to extend only a relatively short distance at each support. With $f_y = 40{,}000$ psi and $f_c' = 4000$ psi, the theoretical length of distribution is shown to be $0.04L$ for $p = 0.025$ (2.5%) and only $0.015L$ for $p = 0.01$ (1.0%).

The authors readily concede that a different theoretical concept might be developed that would fit the evidence, but Mr. Sinha's viewpoint certainly does not fit all of the demonstrated physical action of continuous structural concrete. Nevertheless, he should be encouraged by those having confidence in his approach to add to the experimental background of literature in this area in confirming or refuting his beliefs.

Discussion of a paper by Victor F. Leabu:

Problems and Performance of Precast Concrete Wall Panels*

By JOHANNES MOE, W. H. F. SAIA, and AUTHOR

By JOHANNES MOE†

The author is to be commended for his interesting paper, which is dealing with a problem of great practical value.

Unfortunately the mathematical derivations on pp. 293 and 294 are unclear and partly incorrect, as will be shown below.

Support Condition 1

It should be realized that the forces P and the moment M of the author's derivations are only fictitious quantities of such magnitude as to cause the same deformations as those caused by the differences in temperature between the two faces of the panel. It should also be noted that the author has assumed equally large positive and negative changes in temperature on the two respective faces. The solution derived is therefore not general, but applies to one special case. Different amounts of change in temperature on the two faces would have caused longitudinal forces in the wall panel.

The author's equation for M should be

$$M = Pd \text{ (instead of } M = Pe)$$

It is not necessary to introduce the fictitious quantities mentioned above, since the deformations can be estimated from geometrical considerations only.

Consider the special case treated by the author. Fig. A shows an

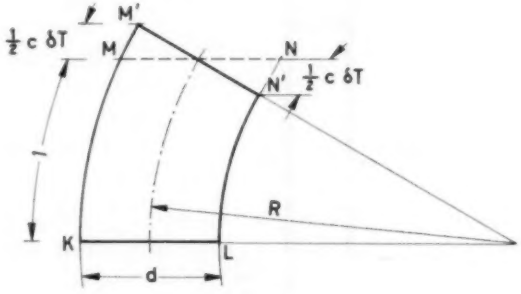

Fig. A

*ACI JOURNAL, V. 31, No. 4, Oct. 1959 (*Proceedings* V. 56), p. 287. Disc. 56-20 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 31, No. 12, June 1960 (*Proceedings* V. 56).

†Member American Concrete Institute, Civil Engineer, Bestun, Norway.

initially straight member (KLMN) of unit length. Due to a temperature difference δT between the two faces, one cross section MN has rotated to the new position M′N′ with respect to the other (KL). It is easily seen that the curvature due to the temperature effect is

$$y'' = \frac{1}{R} = \frac{c\,\delta T}{d}$$

Integrating this equation twice (for instance by the method of a conjugate beam) yields for the displacement of the center of the panel the following expression

$$\Delta = \frac{c\,\delta T\,L^2}{8d}$$

which agrees with the author's formula.

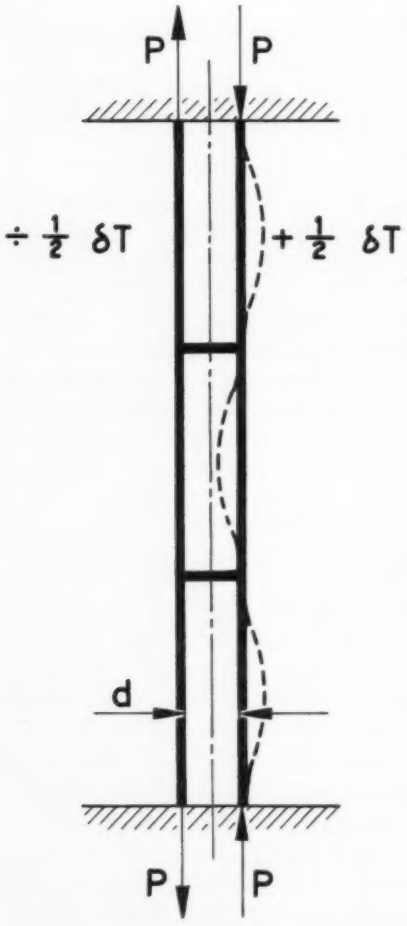

Fig. B

Support Condition 2

This case is incorrectly treated by the author. Contrary to the first case, forces are this time introduced in the panel due to the temperature differences. Full fixity at the ends implies no rotation of the end cross sections, and hence no relative movements of the two faces of the panel in the direction of its length at the end cross sections.

Equally large tensile and compressive forces will therefore result in the two sides of the panel throughout its length. These forces are determined from the condition of no longitudinal displacements in the panel, as follows (Fig. B):

$$\frac{P}{EA/2} = c\,\delta T/2$$

from which

$$P = \tfrac{1}{4}\, c\,\delta T\, A\, E$$

The clamping moment as well as the bending moment throughout the panel equals

$$M = P\,d$$

No displacement perpendicular to panel will result in this case.

However, it is possible that the compressive force on the warm side of the panel may cause local buckling between the ribs, as indicated by a dotted curve in Fig. B.

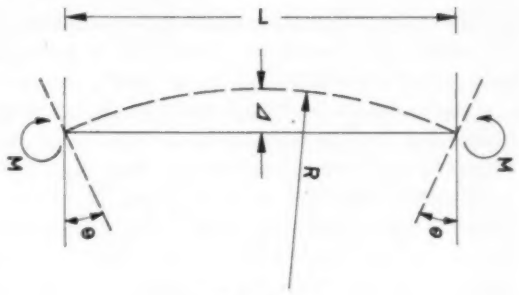

Fig. C

Considering the present problem, full fixity is hardly ever encountered in practice. It may be of interest to note that the values of the center displacement given in Table 1 for Support Condition 2 are correct for three-quarters of full fixity. The form of the displacement curve shown on p. 294 for this condition is, however, not correct since it indicates two points of inflection. The real curve will have a constant radius of curvature R (Fig. C) given by the equation

$$R = \frac{4\,d}{c\,\delta T}$$

The bending moment will in this case be constant throughout the length of the panel, and equals

$$M = \tfrac{3}{4}\,P\,d = \tfrac{3}{16}\,c\,\delta T\,AEd$$

The angle of rotation at the supports is

$$\theta = \tfrac{1}{8}\,c\,\delta T\,\frac{L}{d}$$

By W. H. F. SAIA*

Mr. Leabu is, in my opinion, well qualified from both a technical and experience standpoint to present the viewpoints given in his paper. He is, also, intimately familiar with a serious problem on a panel job where connection failures occurred. I urge him to write a paper describing the connections, failures, and remedies applied.

Information presented (pp. 290-291) relative to insulating characteristics lacks adequate substantiation. The curve in Fig. 3 invites explanation to justify the serious differences between the two high points, particularly since they are equidistant from the rib. If any conclusions are drawn or assumptions made from Fig. 3 data, they must be held in

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proper value. Such conclusions or assumptions, to a penetrating reader, are obviously based on data that is considerably less than conclusive and, most certainly, far from being comprehensive in quantity.

Panels shown in Fig. 4 are not, in my experience, typical. Any panel under 8 x 20 ft would be made without intermediate ribs, except where required by the architect or his structural engineer. Although the arithmetic involved in computing *U* values appears to be correct, job performance seems to refute the calculated data. Insulated sandwich panels in two electrically heated schools have performed close to design, certainly closer than the 40 percent and 50 percent difference mentioned. The schools are using slightly less than computed heat load. Approximately 50 percent of the exterior wall area is insulated sandwich panels exposed directly to exterior and interior temperatures in Michigan.

In the author's computation of temperature effects (p. 292) relative to "bulging," he gives deflections to be expected. Can these effects apply in only one direction under the opposite conditions of heat transfer experienced in summer and winter? The author concedes that "panels seldom curve inward . . ." May I add that no one has ever, to my knowledge, reported a panel curving inward. This leads me to believe the information presented is not conclusive and may even be erroneous.

I wish to take exception to the author's statement regarding vapor barrier characteristics of the insulating core (p. 296). In my opinion, the most commonly used core material is one that has no vapor barrier value.

The author's discussion of curing shrinkage is much too brief. His last sentence (p. 297) is, in my opinion, a serious understatement. Differential curing shrinkage is the biggest problem facing the panel industry. When this problem is solved, I believe the other factors discussed in this paper will become so minor as to be unworthy of consideration.

Although my opinions differ seriously with those of the author, I believe the paper is an important contribution to the panel industry. It is by far the most thought-provoking work I have read on precast wall panels.

AUTHOR'S CLOSURE

The discussion of Messrs. Saia and Moe, together with numerous responses by conference and mail, indicates national and international interest in the problems associated with precast concrete wall panels, and it is gratifying to see that the article was thought-provoking instead of provocative.

The author's original objective was to stimulate interest and present some of the problems encountered with the use of this type of wall

panel to those who are not intimately familiar or lack experience with this type of wall construction.

It is hoped that further investigation and research will be carried on by all those concerned with precast curtain wall fabrication in order to improve this product and correct some of the undesirable characteristics common to this type of concrete construction.

With regard to surface esthetics, it was brought to the author's attention that there is available a dead white cement that has neither a bluish nor yellowish cast.

Having no experience with this cement, the author investigated this type of cement with precast panel producers who have used the dead white. They have indicated that, although it has neither a bluish nor yellowish cast, the dead white does have shades of white similar to the gray natural cements. This characteristic does create a minor problem in maintaining uniformity of the white between adjacent panels.

The many factors that determine an acceptable end result of the esthetic appearance of precast wall panels designed with separate aggregate surfaces, unlimited color combinations, and special textures require skill and experience that should exclude the amateur panel producer.

In response to Mr. Saia and his many observant comments, an attempt will be made to answer some of these comments in the order asked.

The author does not feel that he is qualified in either experience or technical background to write a paper on panel connection failures; however, a few words of wisdom to those who have not been exposed to the many pitfalls associated with panel connections.

Ductility and strength are prime requisites of all good connection material in order to allow for panel movement caused by the many factors discussed in the original article.

Designing the panel connections to have vertical, horizontal, and lateral adjustment has developed into a definite requirement if proper connection of the panels to the supporting frame is to be made economically in the field.

The many types of concrete anchors on the market today lack technical data to substantiate their claims. In many cases, the manufacturer's claims cannot be substantiated as experienced by the author on the connection failure mentioned by Mr. Saia.

It is about time that the precast concrete panel industry, the architects, and the engineers took the initiative to demand that the anchor and insert manufacturing industry furnish the kind of technical data so urgently required for panel connections. Furthermore, the technical characteristics of the concrete anchors should be established by scientific

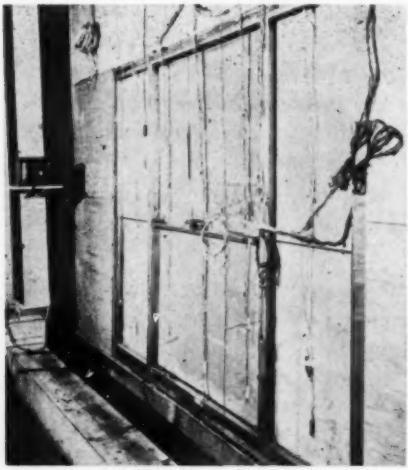

Fig. D—Precast wall panel 4 ft wide x 7 ft high in a climatometer room. Thermocouples are mounted in center panel to measure temperature differential. Dial gages set on cross bar support measure lateral movement of panel during temperature cycles

methods and promotional claims be supported by unbiased laboratory testing data.

It might also be mentioned that corrosive resistant material for anchorages are also finding prominence to prevent failure of the panel connections in the later years due to corrosive action caused by condensation around the anchorage.

With regard to Mr. Saia's apprehension of the accuracy of Fig. 3 and the heat loss through the ribs, the curve shown was taken from actual test data compiled by the Engineering Experiment Department at Pennsylvania State University.*

The slight discrepancy of 3½ deg F between the two high points could be due to the marginal temperature loss at the mullions since the curtain wall tested was not infinitely long but consisted of two side panels 2 ft wide x 7 ft high and a center panel 4 ft wide x 7 ft high separated by aluminum mullions with intermediate concrete ribs (see Fig. D and E).

Although the panel tested was not exactly comparable to those discussed in the paper, it was of the sandwich type made with two layers of lightweight concrete with an insulation core. The test data obtained definitely concluded that solid ribs do create a heat loss that should be calculated in determining the over-all *U* factor of the insulated sandwich panel.

The computed *U* factors, given in Fig. 4, were based on the absolute heat losses through the solid ribs and through the insulated sandwich panel, with no correction made for the marginal heat losses effected by the ribs.

The author has had the opportunity to review many precast concrete designs of supposedly insulated sandwich panels designed by architects, engineers, and the panel fabricators. From observation, he can only conclude that technical "know-how" and experience in many designs were lacking. In some cases, only 50 percent of the panel area was of

*"The Performance of a Curtain Wall Panel Faced with Ceramic Tile for Kermos Panel Co., Dearborn, Michigan," Engineering Experiment Department, Pennsylvania University, Nov. 25, 1957.

sandwich construction and the rest was solid concrete, yet the *U* factor was calculated for the over-all panel based on the insulated section only. Solid concrete ribs 12 in. to 18 in. were not unusual in many panel designs no larger than 4 x 8 ft.

Although the discusser has had experience in actual performance of panels with regard to heat losses, it must be realized that not all concrete curtain walls require the *U* factor specified. In many cases, the insulated sandwich panel could have been solid concrete when used with a masonry backup or plastered inside facing. This backup construction material, sometimes ignored in computing heat losses through walls, makes up for the heat losses not computed through solid rib sections of the curtain wall resulting in a satisfactory performance with relation to the designed heat loads.

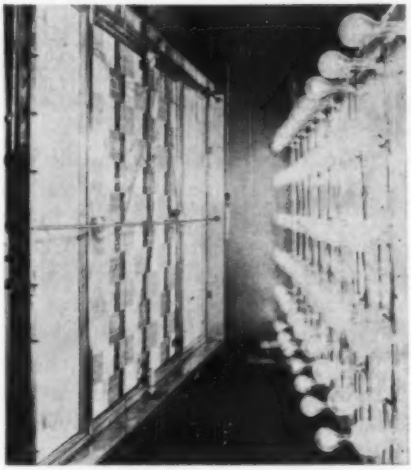

Fig. E—Outside face of ceramic faced precast panel mounted in a climatometer room. Radiation lamps are used to simulate the temperature differential between outside and inside face panel

The fact that most precast insulated sandwich panels have been observed to curve outward regardless of whether the hot face of the panel is on the outside or inside of the building is, of course, limited to the observations of the discusser, and is controversial to say the least. In the author's opinion, this issue can only be settled by proper full size panel testing under simulated climatic exposure and with equivalent panel support conditions.

It is important to note, however, that a few factors contribute to the predominant outward movement of the panels, namely:

(a) Most precast panels are restricted from inward movement by the supporting frame such as floors, roofs, or columns when the temperature cycle is reversed.

(b) Panels are usually designed structurally to span between supports, thus inducing compressive stresses in the top or bottom of the panel. The lack of lateral support, together with the temperature differential, tends to bow the panel toward the direction of minimum resistance which is generally outward.

There are several core materials on the market today that are neither vapor barriers nor even moistureproof; however, there are a few well known and widely used insulation materials that are not only vaporproof but also moistureproof, such as Foamglas.

The author agrees with Mr. Saia that the discussion of curing shrinkage was brief. Many papers have been written on the problem of shrinkage in concrete and probably many more will be written in the future, and the problems will still exist to some extent as long as concrete is used as a material.

The curing shrinkage on the type of concrete panel discussed was not as important as some of the other problems presented.

The author was evaluating the typical sandwich panel with a symmetrical cross section, reinforcing on both faces, and with quality controlled concrete properly cured. All these factors can't help but minimize differential shrinkage.

For a more comprehensive study of warping of concrete due to shrinkage, the author refers the reader to articles such as "Warping of Reinforced Concrete Due to Shrinkage," by Alfred L. Miller [ACI JOURNAL, V. 29, No. 11, May 1958 (*Proceedings* V. 54), pp. 939-950] and also the discussion of the above article in the ACI JOURNAL, V. 30, No. 6, Part 2, Dec. 1958 (*Proceedings* V. 54), pp. 1393-1402.

New trends in panel construction such as panels of unsymmetrical sections, separate aggregate surfaces with a high cement content having a higher shrinkage coefficient than the regular concrete backup, and separate aggregate surfaces with 4 to 6 in. of foam concrete backup that has a much higher shrinkage coefficient than regular concrete create important factors that must be evaluated with regards to curvature and warpage in precast concrete panels due to differential shrinkage.

With reference to Mr. Moe's comments on Support Condition No. 1 and the use of "fictitious derivations," apologies are in order for the typographical error in the use of e in place of d in the equation that should read $M = Pd$. This is incorrectly shown on p. 293; however, the computed deflection of the panels due to temperature differential for Support Condition No. 1, as given on 294, is correctly presented and also agrees with Mr. Moe's derivation.

The disagreement between Mr. Moe's analysis of Support Condition No. 2 and the author's presentation is primarily due to differences in basic assumptions.

The fixed panel condition simulates the interior span of a larger wall panel that is supported vertically and horizontally at the columns but has intermediate ties at the ribs for lateral support as shown in Fig. 6.

The author has based his analysis on the deflection produced in the panel between the ribs at intermediate supports by computing the lateral forces on the ties caused by the temperature differential since the panel cannot move inward or outward at these points.

Mr. Moe appears to have assumed no lateral support at the ribs, and has derived the radius of curvature and the moment on the basis of

full fixity of the over-all panel at the main supports. It must be realized that the intermediate supports can only provide lateral resistance to the panel but does allow rotation of the panel in the vertical plane.

The buckling of the panel faces between ribs or supports due to temperature differential is problematical since most panel designs use shear ties between the panel faces causing the faces to act as a unit similar to a Vierendeel truss.

It is interesting to note that in spite of the basic differences in assumptions, the theoretical computed deflections are not too far apart; however, the author feels that only proper testing under simulated conditions will prove the validity of these theoretical approaches.

In conclusion, the author would like to suggest that the precast panel industry can improve and remedy many of the problems that were emphasized in this paper if they adopt methods of testing panels similar to the metal curtain wall industry. The many problems confronting the metal curtain wall 10 years ago are gradually finding solution by resorting to the proper climatic testing in pressure test cells or climatometer rooms shown in Fig. D and E. These test cells or climatometer rooms permit full size panels to be tested for such items as insulating value, air and water leakage through panels and joints, curvature due to pressure and temperature differential, and even shrinkage due to differential weather exposure can be successfully tested in this manner.

Discussion of a paper by Bill G. Eppes:

Comparison of Measured and Calculated Stiffnesses for Beams Reinforced in Tension Only*

By C. BERWANGER, W. T. MARSHALL, A. SIEV, and AUTHOR

By C. BERWANGER†

I found Professor Eppes' paper on stiffness in reinforced concrete beams very interesting. It attempts a solution of the problem that faces anyone when computing deflections or curvatures for comparison to values from beam tests. This situation presented itself in the recent work for my MS thesis at Queen's University, Kingston, Canada.[8]

This work showed that plastic design methods can be adapted for use with reinforced concrete continuous beams, modifications, of course, being necessary because of the limited ductility of reinforced concrete sections. The results of these tests were presented in a paper at the ASCE Los Angeles convention, February, 1959.[9]

Two series of beams were tested. The first consisted of two simply supported beams. The main series was six, two-span continuous beams with concentrated loads in each span symmetrical about the center support. Fig. A shows details of these beams. The beam testing arrangement is shown in Fig. B.

The beam cross sections were 3 x 6.5 in. and were reinforced with #3 and #4 high-bond bars in varying percentages. Tension tests showed a characteristic stress-strain curve with no definite yield plateau, but a tendency to strain harden once yielding had occurred. Table A gives average values for the two bar sizes used. High-early-strength portland cement was used for all beams. Huggenberger mechanical

TABLE A—TENSION TESTS OF REINFORCEMENT

Bar size	A_s, sq in.	f_y, ksi	ε_y, 2-in. gage	E_s, psi	f_u, ksi	ε_{su}, 8-in. gage	E_{sh}, psi
#3	0.1136	44.6	0.00166	26.4×10^6	75.5	0.20	0.98×10^6
#4	0.193	49.5	0.00193	30.8×10^6	78.9	0.177	0.76×10^6

*ACI JOURNAL, V. 31, No. 4, Oct. 1959 (*Proceedings* V. 56), p. 313. Disc. 56-22 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 31, No. 12, June 1960 (*Proceedings* V. 56).

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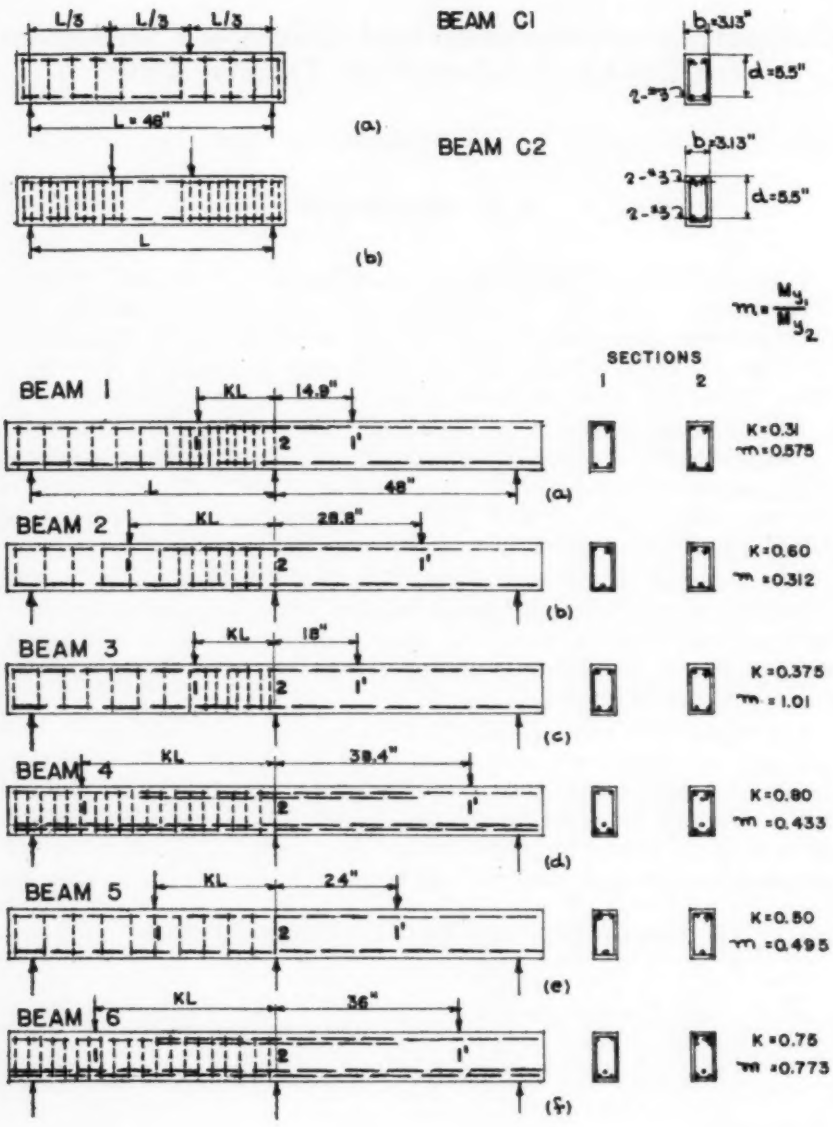
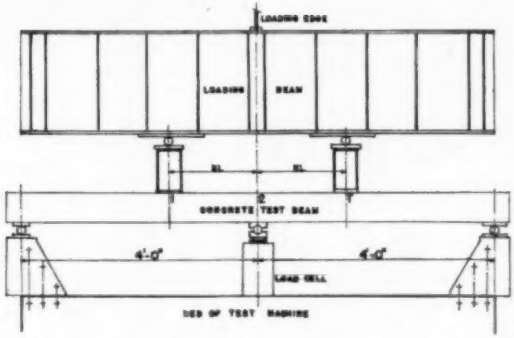


Fig. A—Beam details

Fig. B — Beam testing arrangement



strain gages with a 1-in. gage length and a magnification of 1200 were used to measure steel and concrete strains. Concrete mix data are given in Table B with beam cross section properties presented in Table C.

Moment-curvature and flexural rigidity-moment relationships plotted from the experimental results of Beams C1, 1, and 5 are presented in Fig. C, D, and E, respectively. Beam C1, simply supported, failed in tension with ultimate crushing of the concrete at midspan. Continuous Beam 1 failed initially by yielding of the steel at Section 2, at the center support, then the steel yielded at Section 1, under the load, and crushing of the concrete at Section 1 at ultimate load. Beam 5 failed initially in tension at Section 1, then at Section 2, and ultimate failure by concrete crushing at Section 1. No shear failures occurred in any of the beams tested.

It will be noticed that the midspan section of C1; Section 1 of Beam 1; and Section 2 of Beam 5 are similarly reinforced with two #3 bars in tension. The extra #3 bar in compression at Section 2, Beam 5, will affect the ultimate moment capacity and curvature to some extent.

TABLE B—CONCRETE MIX DATA

Beam cross section	f_c' on day of test	f_c' at 7 days	Beam age on day of test	E_c, psi
C1—midspan	3560	3000	10	3.44×10^6
C2—midspan	4030	4030	7	3.65×10^6
#1—1	6200	4330	13	3.75×10^6
#1—2				
#2—1	5700	5200	13	3.80×10^6
#2—2				
#3—1	4000	3700	13	3.05×10^6
#3—2				
#4—1	4550	3700	12	5.35×10^6
#4—2	5500	5330	12	4.85×10^6
#5—1	2300	1980	11	3.35×10^6
#5—2	3400	2430		
#6—1	4900	4400	9	3.78×10^6
#6—2	5000	4550		4.95×10^6

TABLE C—PROPERTIES OF BEAM CROSS SECTIONS

Beam cross section	k Load position	b, in.	d, in.	Steel tension bars	Steel compression bars	p	p'	n	n'	q_v	q_a	q_v'
#1—1	0.31	3.05	5.21	2 #3 10	—	0.0143	—	6.85	—	0.100	0.124	—
#1—2		3.05	5.55	2 #4 1	2 #3 10	0.0224	0.134	8.73	6.85	0.165	—	0.115
#2—1	0.60	3.09	5.46	1 #3 3	—	0.0068	—	6.00	—	0.052	0.058	—
#2—2		3.08	5.50	2 #4 1	1 #3 3	0.0223	0.0068	7.40	6.00	0.179	—	0.158
#3—1	0.375	3.03	5.39	2 #3 5	2 #3 4	0.0141	0.0140	9.00	8.82	0.153	—	0.073
#3—2		3.03	5.30	2 #3 4	2 #3 5	0.0140	0.0141	8.82	9.00	0.157	—	0.084
#4—1	0.80	3.08	4.83	3 #3 6	—	0.0229	—	5.15	—	0.214	0.332	—
#4—2		3.05	5.43	3 #4 2	3 #3 6	0.0352	0.0206	5.95	5.15	0.343	—	0.215
#5—1	0.50	3.00	5.34	1 #3 8	—	0.007	—	9.22	—	0.140	0.172	—
#5—2		3.07	5.50	2 #3 8	1 #3 8	0.0136	0.0070	7.55	9.80	0.183	—	0.156
#6—1	0.75	3.07	5.20	3 #3 9	—	0.021	—	7.35	—	0.200	0.218	—
#6—2		3.08	5.28	4 #3 11	3 #3 9	0.0278	0.0208	5.9	5.6	0.256	—	0.124
C1	1/3 points	3.125	5.50	2 #3 1	—	0.0134	—	7.56	—	0.165	0.201	—
C2	1/3 points	3.125	5.50	2 #3 2	2 #3 2	0.0134	0.0134	6.82	6.82	—	—	0.075

Table D presents calculated values of EI for four methods of computing I. The initial tangent modulus, E_c, determined experimentally was used in the products.

For the simple beam, the upper range of values seems best defined by I based on the effective cross section (bd) and the lower range by the transformed section. This is also the case with Section 1, Beam 1, and Section 2, Beam 5 although the curve for this section is much flatter than the other two.

For Section 2, Beam 1 the upper range can be given by I computed from the gross section plus transformed steel area with the lower range given by the transformed area method. However, for Section 1, Beam 5, the gross area seems closest for the upper range with the transformed section for the lower range.

Compare the tension reinforced sections at midspan, for the simple beam, and under the load in the continuous beams. The lower range of values can be given by the transformed section in all cases, and the upper range by either the gross area or the effective cross section area. The sections with both tension and compression reinforcement Section 2, have the transformed section for the lower range of values and the effective cross section area or the gross area with transformed steel area for the upper range.

The transformed section method of computing EI for the lower range of values seems best. No conclusions can be drawn for the upper

TABLE D—COMPUTED VALUES OF FLEXURAL RIGIDITY

Beam cross section	Effective cross section bd	Gross cross section	Gross section transformed steel area	Transformed section
C1—midspan	154×10^6	255×10^6	284×10^6	105×10^6
#1—1	135×10^6	268×10^6	285×10^6	91×10^6
#1—2	166×10^6	268×10^6	312×10^6	178×10^6
#5—1	126×10^6	229×10^6	238×10^6	60×10^6
#5—2	143×10^6	240×10^6	280×10^6	101×10^6

range. However, the effective cross section area method could be taken as a reasonable average value for EI of tension reinforced sections. No conclusions, of course, can be drawn for the tension and compression reinforced sections.

The EI-M curves presented do not have the upper levelling off shown in Professor Eppes' paper. It is difficult at times to interpret the points since there is a large amount of scatter with much of it occurring in the early stages, when the experimental error would be a major factor. The maximum value of EI for the uncracked section will apply over a small range of moments especially in beams with small percentages of steel. It was stated by Professor Eppes as being about $0.20M_u$ based on the simple beam tests by Gaston, et al.[6] It would seem to be much less than this at times in a continuous beam.

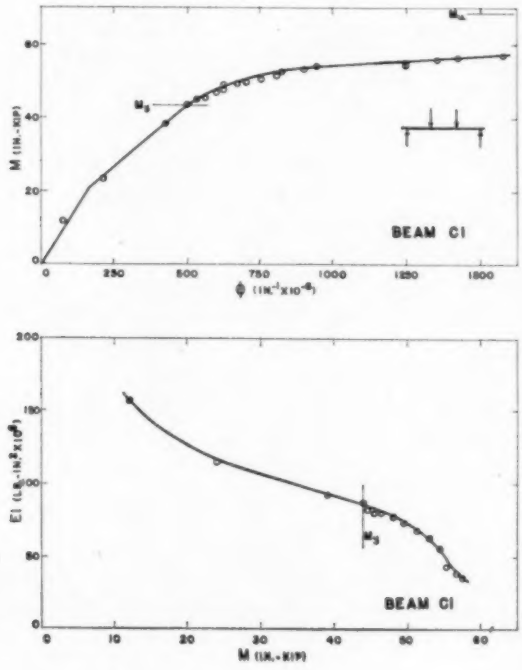


Fig. C — Moment-curvature and flexural rigidity-moment relationships

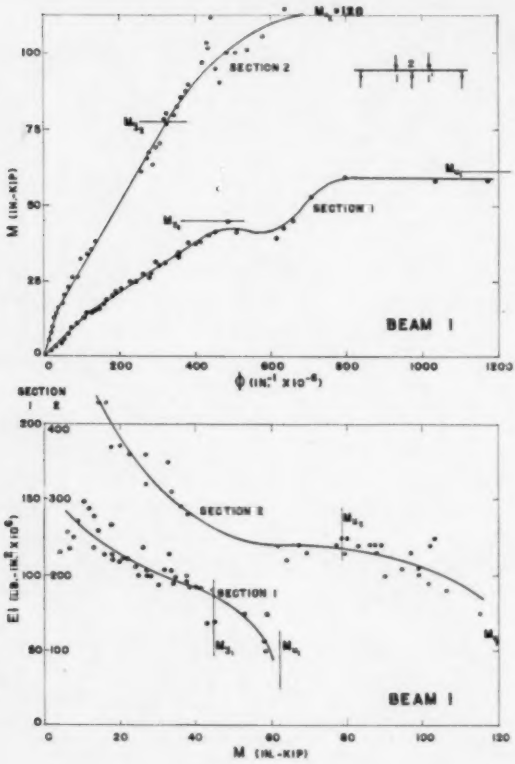


Fig. D — Moment-curvature and flexural rigidity-moment relationships

Strains were taken as close to ultimate load as possible and there is usually a rapid dropping off of the *EI* curve as the yielding of the tension steel takes place.

The *EI-M* relationship for continuous beam critical sections, will reflect the beam capacity for moment redistribution as well as its mode of failure. They will modify to some extent the curves as determined for similar sections in a simple beam.

These two effects may possibly have produced the flatter curve for Section 2, Beam 5, although the compression bar may have had a greater effect in this direction. The results presented are meager but may be of some value in any future studies of this topic.

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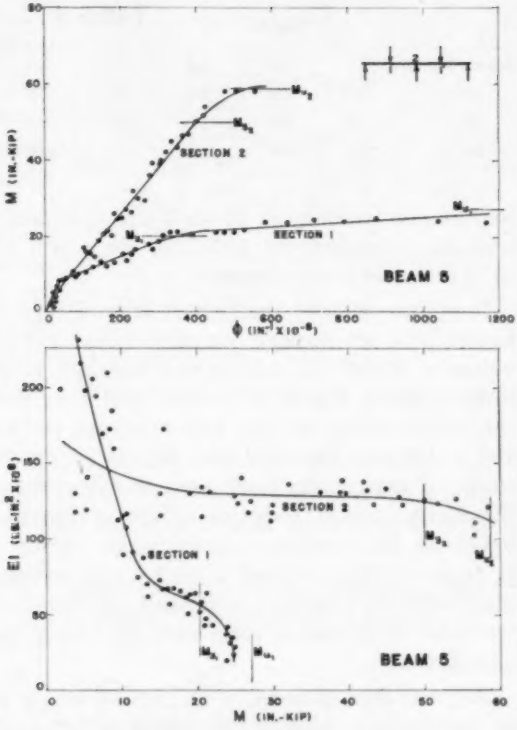

Fig. E — Moment-curvature and flexural rigidity-moment relationships

By W. T. MARSHALL*

The writer was interested to read the paper by Mr. Eppes since he had done some work on a similar subject some years ago.† The experimental results showed certain characteristics similar to those of the author, namely a lowering of the *EI* value as the moment is increased and a value at working load greater than that of the transformed section neglecting the concrete below the neutral axis.

The writer from his experiments concluded that for both singly and doubly reinforced beams the flexural rigidity at working load varied from 1.20-1.33 times the flexural rigidity of the transformed section. If, however, Mr. Eppes' results are analyzed, the working load being taken at the value given in Table 1, then the values for *EI* at working loads scaled from Fig. 6 and 7 are given in Table E where EI_W denotes the flexural rigidity at working load. The values of *EI* for the net

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†Marshall, W. T., "The Flexural Rigidity of Reinforced Concrete Beams," *The Structural Engineer* (London), V. 23, No. 4.

TABLE E

Beam No.	1.1	1.2	1.3	1.4	2.1	2.2	2.3	2.4	2.5
EI_W	290	290	250	260	300	400	280	290	300
EI_{NA}	148	207	190	246	150	150	205	251	251
Ratio	1.97	1.45	1.32	1.06	2.00	2.67	1.37	1.16	1.20

section are taken from Table 2 and it is seen that the ratio between EI_W and EI_{NA} varies from 2.67-1.06. This is a much greater variation than in the writer's experiments.

It is also noticed that apart from Beam 2-2 the experiments by Mr. Eppes give an almost constant value for EI_W of 290 $\times$ 10^6 psi. The value of $Ecbd^3/12$, taking the average value of E_c obtained from the tests given in Fig. 8, $b = 6$ in. and $d = 5\frac{5}{16}$ in., is 278 $\times$ 10^6 psi while the value based on the full concrete section, i.e., when $d = 6$ in., is 406 $\times$ 10^6 psi. The fact that EI_W is reasonably consistent for the whole range of experiments supports the first recommendations of the British Standard Code of Practice which is that the moment of inertia can be based on the concrete section alone, while ignoring the reinforcement. It supports the method used by the writer in his contribution to the discussion on a paper by Hansell and Winter* where the lateral instability load of beams is estimated by using the concrete section alone in calculating I.

A great deal of work is at present being carried out on plastic design in reinforced concrete. For this design method to be used effectively the flexural rigidity of a reinforced concrete member must be known. Mr. Eppes has therefore carried out experiments which will be useful in the development of plastic design.

By A. SIEV†

The use of the effective (stiffness) value of a member recommended by the author on the basis of the agreement between calculated and measured values seems to have certain additional implications, given the following assumptions:

(1) The steel cross section varies along the span and follows the bending moment diagram [Fig. F(b),(c)].

This is ordinarily the case in practice, especially in long-span and heavy beams. For simplicity, let us also assume continuous instead of discrete variation.

(2) The stiffness of a cracked section is proportional to the steel cross section, other conditions being equal.

*Marshall, W. T., Discussion of "Lateral Stability of Reinforced Concrete Beams" by William Hansell and George Winter, ACI JOURNAL, V. 31, No. 9, Mar. 1960 (*Proceedings* V. 56), pp. 958-959.

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The figures in Table F (see Table 2 in paper) show that deviation from the mean value of M/EI is less than 10 percent for the steel percentage in question.

TABLE F

Beam No.	Percentage of steel	EI	EI/P
1—1	0.692	148×10^6	215
1—2	1.04	201×10^6	194
1—3	0.96	190×10^6	197
1—4	1.385	246×10^6	178
2—1	0.692	150×10^6	217
2—3	1.04	205×10^6	197
2—4	1.385	251×10^6	181

It can easily be shown theoretically that the ratio M/EI is by the above assumptions, proportional to $(1 - k)d$ which may be considered almost constant in the normal range of beam reinforcing. It appears then that the absolute value of the curvature along the beam is also constant.

The stiffness of cracked section is almost proportional to percentage of steel.

First example — A rectangular reinforced concrete beam fixed at both ends is loaded arbitrarily (Fig. F). Assume that the *real* moment distribution (effective values taken into account) is as shown in Fig. F(b).

The M/EI diagram [Fig. F(d)] comprises two lines indicating the same constant absolute values except near the point of inflection C, where stiffness increases owing to the smallness of the moment. As a small section of a curve may be assumed linear, the curve near A is antimetric. The deflection curve in Fig. F(e) comprises three sections AB, BD, and DE. AB and DE are of constant and equal curvature, while BD is of variable curvature antimetric about C. Simple geometry shows that the point of inflection C is at quarter-span, irrespective of load distribution.

In the case of a uniformly distributed load, the positive moment will be $+ ql^2/32$, and the negative $- 3ql^2/32$.

However, if the steel cross section is relatively reduced in the negative moment zone, the beam stiffness will follow accordingly, and the moment of restraint will decrease similarly.

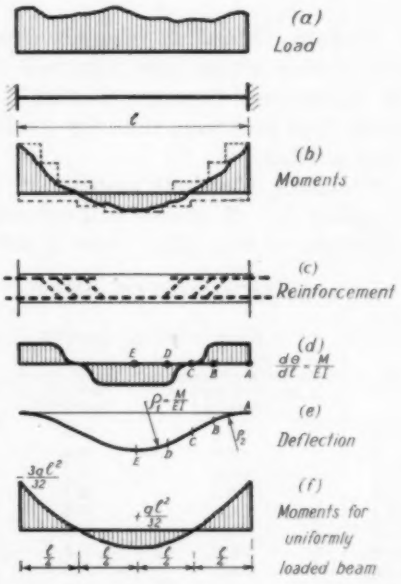

Fig. F — Calculation by the effective value of a member

Second example — Same as before except that the beam is a T-section. In this case AB and DE will be constant but different in curvature. It is clear that the stiffness of ED is higher than that of BA—for cracked sections—and $\rho_1 < \rho_2$, so that C moves to the right and the negative moment will be much less than in the conventional case. This result is very important, since the negative moment is often the one dictating the beam depth or the enlarged cross section of floor ribs near the supports. (The possibility of arriving at the same conclusions by means of the plastic theory is not discussed here.)

In this context, the fact that the measured stiffness of cracked beams is higher than the theoretical one (probably owing to the contribution of the stressed concrete in the tension zone between the cracks) seems to be of great importance, since this effect is likely to be more pronounced in T-beams.

It should be of interest to study the following in addition to points mentioned in the paper:

(a) The stiffness of T-beams in positive and negative moments.

(b) The actual state of stress in a section type of the first example.

(c) The effect of plastic deformation on stiffness, with a view to calculating the moment distribution.

AUTHOR'S CLOSURE

Professor Marshall recognized many of the problems pointed out by the author which are discussed in his paper "The Flexural Rigidity of Reinforced Concrete Beams."* The author was not aware of the work that had been done by Professor Marshall at the time the paper was written.

Professor Marshall points out that the value of EI_W is relatively constant for all beams without regard for reinforcing and modulus of elasticity of concrete, which is interestingly enough so. The values of moments of inertia by various methods of calculation are shown in Table G. One will notice that there is not a large variation in the values for the gross section, but there is a large variation in the case of the cracked section. The effect of the steel is more pronounced in the latter case. The average value of E for the first casting (Beams 1-1 through 1-4) is 3.35×10^6 psi while the average value of E for the

TABLE G—MOMENTS OF INERTIA

Beam No.	By gross section including transformed area of steel, in.^{4}	By cracked section including transformed area of steel, in^4
1—1	122.0	35.5
1—2	126.5	48.5
1—3	125.6	45.7
1—4	129.7	59.4
2—1	122.0	35.5
2—2	122.0	35.5
2—3	126.5	48.5
2—4	129.7	59.4
2—5	129.7	59.4

*op. cit.

second casting (Beams 2-1 through 2-5) is 4.02×10^6 psi, and for this reason all beams should not be too closely compared in the same frame of reference.

One may see that the ratios

$$\frac{(EI_W \text{ for Set 1})_{avg}}{(EI_W \text{ for Set 2})_{avg}} = 0.870$$

and that

$$\frac{(E \text{ for Set 1})_{avg}}{(E \text{ for Set 2})_{avg}} = 0.834$$

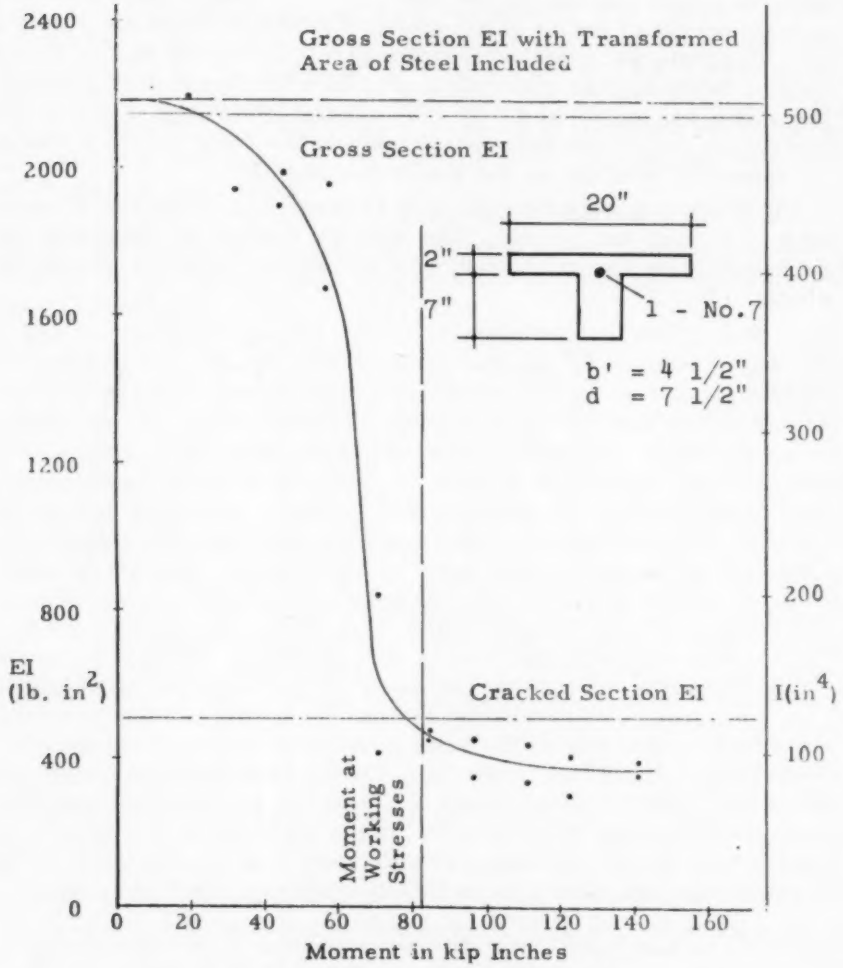

Fig. 6—*M* versus *EI* diagram for T-beam

The author agrees with Mr. Siev that the concrete in tension supplies a significant resistance to bending. The strain in the steel is thus affected causing the calculated values of *EI* for the cracked section to be on the conservative side. The calculated value of *EI* for the uncracked section is also less than the measured value; for this the author knows no immediate explanation.

The author was curious about the *EI* versus *M* diagram for a T-beam in negative bending and has tested one such beam. The results are shown in Fig. G.

Professor Berwanger mentions that his *EI-M* curves do not agree with the author's curves for low values of *M*. Professor Berwanger had a large amount of scatter of experimental points in this range (using a 1-in. Huggenberger mechanical strain gage) as did the author (using an 8-in. Berry mechanical strain gage). In more recent tests, however, the author has employed a 6-in. SR-4 electric strain gage in which the early stage scatter has been considerably reduced and the *EI-M* curves do appear to level out as the author has shown.

The author questions the validity of Professor Berwanger's *EI-M* curve after the steel has yielded. The author's method of measuring *EI* depended upon the reinforced concrete section behaving essentially elastic.

Discussion of a paper by J. Glucklich:

Rheological Behavior of Hardened Cement Paste Under Low Stresses*

By FRANK A. BLAKEY, A. M. NEVILLE, and AUTHOR

By FRANK A. BLAKEY†

Dr. Glucklich has presented some interesting data to add to the knowledge of the properties of hardened cement paste. A thorough understanding of the behavior of concrete under load can only be achieved by an appreciation of its internal structure, which in turn requires a knowledge of the properties of each of the major components, cement paste and aggregate, so that the interaction of one with the other may be understood.

It is of particular interest to find that the author has obtained linear load-deflection relationships for cement paste under instantaneous load. The writer obtained similar results when studying the cracking of concrete beams.[9]

The inevitably long-term nature of creep studies means that each investigator can study only a small number of variables, and the differences in the techniques of the various workers makes quantitative comparison of results difficult. For example, the author has studied the behavior of saturated specimens, whereas the data which the writer has obtained[10] refer to specimens allowed to dry in air. For any comparison of the results, therefore, it is necessary to know to what extent Dr. Glucklich's specimens are behaving as saturated soils, with deformations which may be explained in some way analogous to Terzaghi's theory of consolidation. Such behavior, if it exists, would probably not be important in ordinary air-dried concrete. On the other hand, in using air-dried specimens the writer permitted the influence of atmospheric carbon dioxide, which is certainly of considerable importance in some aspects of concrete behavior.

In the study of the shear strength of beams it has been found that the experimental method of loading greatly affects the results obtained. It is not unlikely that in respect to creep, similar effects have arisen so that in the words of the author, "The problem has generated many, and often contradictory, theories." One instance may be the method of support of beam specimens used for creep tests, and the extent to

*ACI JOURNAL, V. 31, No. 4, Oct. 1959 (*Proceedings* V. 56), p. 327. Disc. 56-23 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 31, No. 12, June 1960 (*Proceedings* V. 56).

†Officer-in-Charge, Mechanics and Physics of Materials, Division of Building Research, Commonwealth Scientific and Industrial Research Organization, Highett, Australia.

which loads concentrated over a very small area may give rise to high local stresses and therefore high creep. The writer has used a technique similar to that of the author in studies of creep of cast gypsum plaster, and found that local deformations at the load points and supports were sufficient to cause errors in the deflection measurements of 20×10^{-4} in. Again it is essential to ensure that the method of the support is genuinely free, and that no horizontal restraints are introduced. Many accepted forms of support in beam tests can in fact introduce horizontal restraints.

These comments are made not as a criticism of the methods used by Dr. Glucklich but as possible explanations of why creep results from different workers have not always been concordant.

The author emphasizes that his study relates to ". . . paste under low stresses," but does not give any indication of just how low the stresses are. The maximum stress used was 25 kg per sq cm, but the question is whether this was one-quarter, one-third, or some other fraction of the ultimate strength.

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9. Blakey, F. A., and Beresford, F. D., "Tensile Strains in Concrete. Parts 1 and 2," *Report* No. C2.2-1 and No. C2.2-2, Division of Building Research, Commonwealth Scientific and Industrial Research Organization (Australia), 1953 and 1955.

10. Blakey, F. A., and Lewis, R. K., "The Deformation and Cracking of Hardened Cement Paste When Shrinkage Is Restrained," *Civil Engineering and Public Works Review* (London), V. 54, No. 635, pp. 577-579.

By A. M. NEVILLE*

Dr. Glucklich's tests make an interesting study, and it would be of help if he could add some more detailed information on his investigation. In particular, I wonder how the strength of the pastes with different water-cement ratios varied. He mentions that the "mobility" decreased with a decrease in W/C at a decreasing rate, and this would appear to confirm my suggestion, made earlier in the ACI JOURNAL,[11] that total creep is proportional to the stress-strength ratio of the specimen. For a decreasing W/C the value of the quotient 1/strength would decrease at a decreasing rate. Dr. Glucklich's observation on the proportionality of time-deformation and load confirms the stress part of my suggested general relationship.

It is hoped that further tests will help us to assign a more specific physical meaning to the reversible and nonreversible parts of the time-deformation.

REFERENCE

11. Neville, A. M., "Role of Cement in the Creep of Mortar," ACI JOURNAL, V. 30, No. 9, Mar. 1959 (*Proceedings* V. 55), pp. 963-984.

*Member American Concrete Institute, Lecturer, University of Manchester, Manchester, England.

AUTHOR'S CLOSURE

The author is glad to have the opportunity to comment upon the interesting and helpful remarks made by Drs. Neville and Blakey — both of whom he considers to be authorities in this field.

The influence of both W/C and load upon the rate of creep (or "mobility" as defined in the paper) was only briefly mentioned. Since the author shares Dr. Neville's opinion as to the importance of this aspect he brings here (see Fig. A) a diagram of rates of creep (for which the slopes of creep curves were measured) versus bending moments as reflected at three different ages for three W/C ratios. It will be noticed that these represent fairly straight lines (a fact that made some investigators attribute creep to viscous flow) which is in agreement with the suggested models. The influence of W/C is as indicated by Dr. Neville; similar to it is the influence of age. Indeed these two factors are complementary in their influence upon various properties of the cement gel, such as density, rigidity, and strength, all of which are connected with the process of continued hydration. Dr. Neville's view as to the interdependence of creep rate and strength is also accepted by the author. Unfortunately, destruction tests on the slender bars used in the investigation resulted in such a scatter of strengths as to be almost of no significance. This was anticipated as a slender bar would break in two at the appearance of the first crack, and its strength would thus be subjected to a severe law of chances. However, compression tests conducted by the author with the same W/C ratios (results not published) positively support this view.

The author cannot but agree with Dr. Blakey as to the importance of the technique employed in a creep test. As long as we have no complete grasp of the mechanism of creep we have to be doubly careful when deciding on such matters as constraints, manner of loading, and hygrometric environment. With regard to the last point, may it be remarked that it is not considered that the specimens were saturated, as was suggested by Dr. Blakey. In spite of the high W/C employed (32 percent) it is almost certain that negative vapor pressures develop with continued hydration due to the prevention of additional water from being absorbed by the specimens. This hygrometric condition is an intermediate state between the two extremes of full saturation and full exposure to the atmosphere. In the case of the former, creep will be a minimum and will mainly be a reversible creep. In the case of the latter, depending on the relative pressures of specimen and atmosphere, creep will be much augmented by ecological shrinkage.

Because the creep investigated was in flexure, it was naturally regarded as creep under low stresses. Very roughly the tensile stresses thus employed were about 50 percent of the tensile strength of the material, this value being dependent on W/C and age.

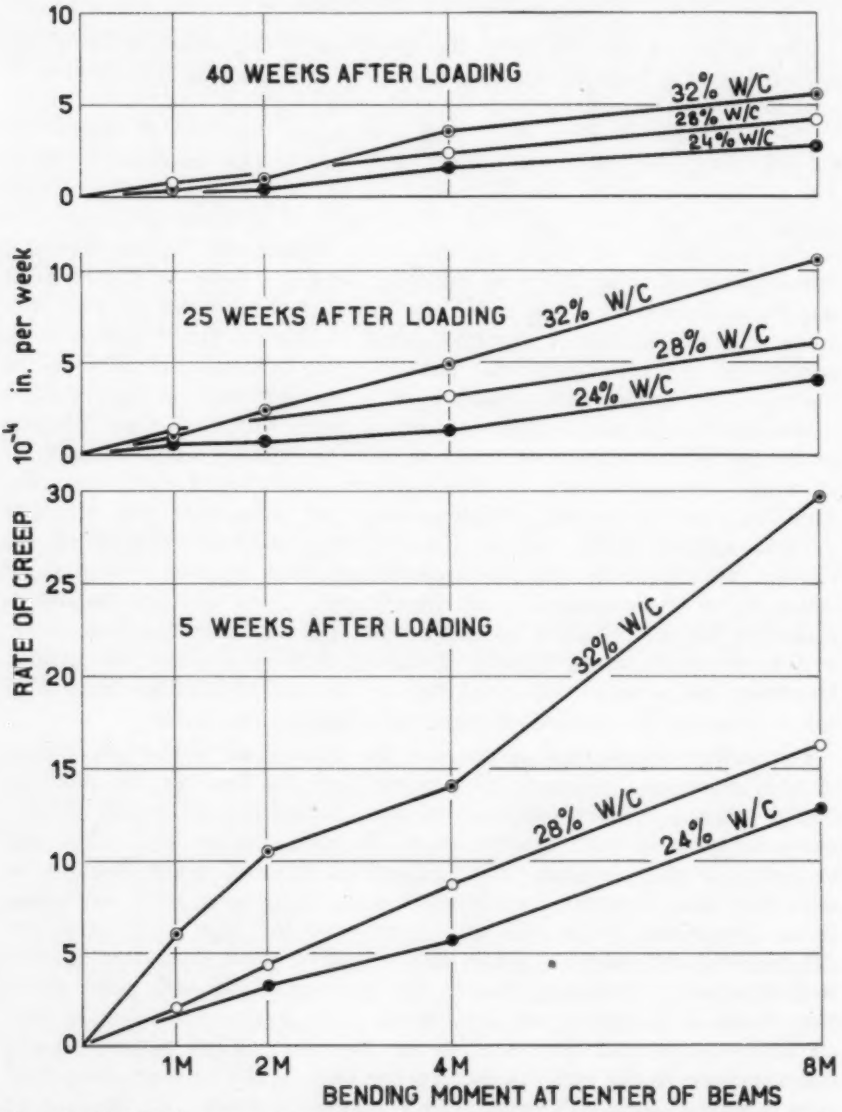

Fig. A—Rate of creep versus bending moments of hardened cement paste beams in flexure. *M* is a bending moment due to the dead weight of the beam = 9100 cm-g (7.93 in.-lb)

Since this paper was published the author has made further tests with the object of acquiring a better insight into the mechanism of creep, and he hopes to have the privilege of presenting it in a future issue of this journal.

Discussion of a paper by M. R. Smith and Gordon M. Kidd:

Concrete Technology and Aggregate Production for St. Lawrence Seaway*

By LEWIS H. TUTHILL and AUTHORS

By LEWIS H. TUTHILL†

Concrete being the generally serviceable and satisfactory material it is for such well-built structures as the authors have so ably described, it is unlikely that within this century, or at a later time, there will appear differences which can be then identified as the results of any of the dissimilarities in concrete practices followed on either side of the river. With the care that both sides undoubtedly devoted to selection, preparation, and use of materials and to the details of good concrete construction, it is predicted the resulting concrete work on both sides will be in generally excellent condition for a very long time.

Differences in specifications and practice, however, have important aspects and significance other than for long range performance of the concrete in service. Prominent among these are factors that affect economy by simplifying engineering and inspection and by affording the bidder assurance that in each step of the procedure he will have a maximum freedom of choice of method and equipment as long as good results are obtained. Also, among these other aspects are factors which make for easier and surer inspection and which insure greater uniformity of results. To an unusually great extent, these objectives have been attained by both sides. A few items that might be worth further consideration will be briefly mentioned.

Long experience has shown that the seemingly attractive advantages of furnishing aggregate to the construction contractor often prove to be the contrary before the job is over. The disadvantages of this arrangement are now looming large for a western municipal project that was ill advised to set up their project in this manner. Engineering and inspection costs are increased. The agency, rather than the prime contractor, is responsible for suitable aggregate as used. The aggregate contractor is not motivated to make unforeseen modifications the prime contractor and the agency may find later to be advantageous. For these reasons, the U. S. Bureau of Reclamation stopped furnishing ag-

*ACI Journal, V. 31, No. 5, Nov. 1959 (*Proceedings* V. 56), p. 361. Disc. 56-24 is a part of copyrighted Journal of the American Concrete Institute, V. 31, No. 12, June 1960 (*Proceedings* V. 56).

†Member American Concrete Institute, Concrete Engineer, California Department of Water Resources, Sacramento, Calif.

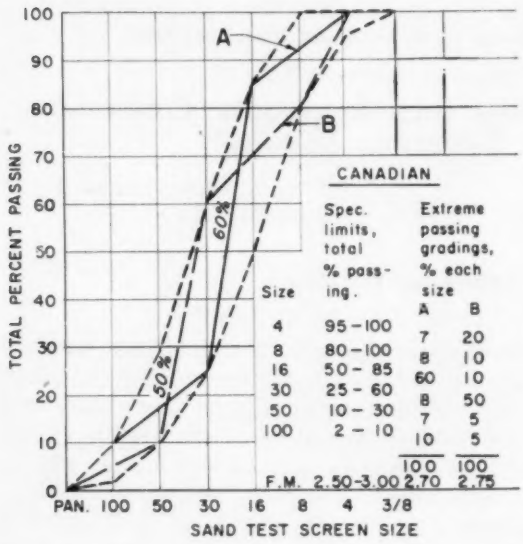

Fig. A(a)—Canadian specification limits for sand grading

gregates about 15 years ago, and were agreeably surprised to be unable to find an increase for the aggregates in comparable concrete unit bid prices thereafter.

Whichever way the aggregate is supplied, there is often far too much spent on investigation of potential sources. Investigation of the most

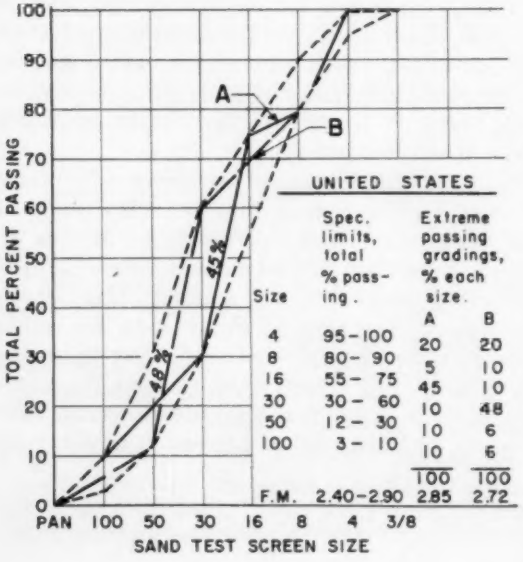

Fig. A(b) — United States specification limits for sand grading

obvious source is certainly indicated, particularly if there are practically no others. Where there are several, it has often proved impossible to predict what the contractor will do, and a considerable sum may be essentially wasted in trying to cover all the possibilities. Beyond study of the one or two most likely and acceptable sources, it would be more economical to require the successful bidder to designate his source of aggregate at the time the contract is awarded. There is usually time then, before concrete work will start, to determine whether the material is acceptable; and, if it is not, at least the one already tested can be used.

With some differences in limits, both sides used a sand grading specification in terms of total percent passing the usual U. S. Standard square mesh screen sizes. Widely used by many agencies and highway departments, such a method of specifying sand grading limits is ordinarily satisfactory, although some highly unsatisfactory gradings would meet such a specification. Few engineers apparently realize that such limits as the Canadians used would permit as much as 50 percent of the sand to be between No. 16 and 8 mesh, or 60 percent between No. 30 and 16, or 50 percent between No. 50 and 30. Similarly, the United States limits would permit as much as 45 percent between No. 30 and 16, or 48 percent between No. 50 and 30. Fig. A(a,b,c) shows this graphically in comparison with a grading specification which places limits on the amount of the sand which may be retained on each screen. This is much more specific and from it all concerned have a much clearer understanding of the true grading of the sand; that is, the relative amounts of each size. With such a specification, no sand meeting it can be as excessive in any one size as

RECOMENDED

Size	Percent each size Spec. limits	Extreme passing gradings A	B
4	0 – 5	2	0
8	5 – 15	5	15
16	10 – 20	20	10
30	20 – 30	20	30
50	25 – 35	35	25
100	12 – 20	12	17
PAN	3 – 7	6	3
		100	100
F.M.	2.50 – 3.00	2.59	2.72

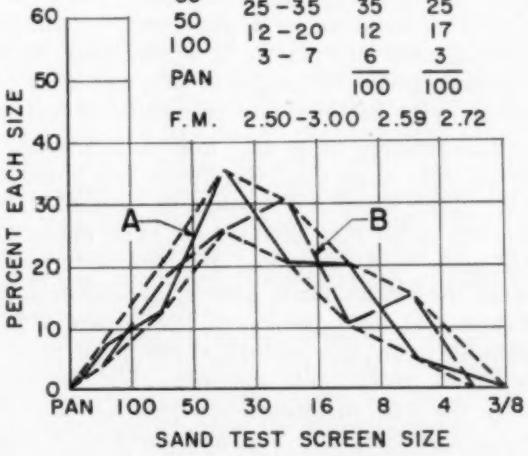

Fig. A(c)—Recommended specification limits for grading

is possible in most total percent passing (or retained) specifications. Amounts outside the limits for each size are permitted only if they produce equally workable concrete without increasing unit mixing water content. Usual requirements as to range of fineness modulus and uniformity can be included.

Methods of cleanup of horizontal constructions joints, other than by wet sand blasting, are probably less costly than wet sand blasting only when they are permitted to produce a cleanup that is inferior to sand blasting and washing the last thing before concrete is placed. For this reason, other methods require more attention and inspection than sand blasting, if equivalently complete and unmistakably thorough and uniform cleanup is obtained. With sand blasting, it is easy to get such results, and inspection is accordingly simplified.

Chutes and belts are often a most economical element in concrete handling and placing facilities. To prohibit their proper use probably adds something to equipment costs. Actually, little is accomplished by prohibiting them because separation does not occur in a chute or on a belt; it takes place as concrete leaves the ends of these devices, just as it does at the end of very short chutes from mixers and hoppers. What is important is to require that separation be corrected at these points, as it is possible and practical to do if the layout is planned that way in the beginning. Any clusters of coarse aggregate still appearing in the concrete as placed should be scattered before vibration or before it is covered with new concrete. Proper vibration will then make these rocks a proper part of the concrete mass.

Presumably, the performance of mass concrete in Ontario has been good enough to justify the casual Canadian approach to temperature control of mass concrete. If so, and this should be a matter of verifiable fact and not of opinion, it would seem that, to some extent at least, the restrictions on height of lifts and time between them south of the river were unnecessarily restrictive in that climate area.

Nevertheless, it is felt that in omitting 3- to 6-in. rock from the north side mass concrete, there was more involved than extra plant cost being offset by savings in cement. First, it is doubted if costs for the extra rock size would have approached the saving in cement for such large volumes. But unmentioned and apparently unvalued were the reductions in heat of hydration and temperature rise that the 0.39 to 1.44 sacks per cu yd less cement would produce, as well as the benefit to drying shrinkage resulting from considerably less mixing water in the concrete with 6-in. rock. In Table 1, there is no indication why the 3-in. maximum concrete for the Iroquois Division, using the same aggregate and the same amount of cement, required about 50 lb more water per cubic yard with ½-in. less slump than the B4.1 mix with 3-in. rock.

With modern vibrators for mass concrete, including an available one-man type, being fully capable of consolidating a full 20-in. layer and somewhat below it, requirement of placement in 12-in. layers seems uselessly restrictive. Advancing, unsupported edges of the layer should not be vibrated until an adjacent batch has been placed to support it. In this way, the unsuitability of slope vibration is avoided and an efficient placing operation is permitted.

The same curing effort is not required in winter and summer because the rate of moisture loss is so little in winter. Freezing weather complicates water curing. In the climate of this project, it would seem adequate and less restrictive to permit curing with a sealing compound in winter. In nonfreezing weather, it is usually practical and worthwhile to use water curing for mass concrete, at least for 7 days, inasmuch as cleanup operations supply an appreciable portion of it.

CLOSURE by M. R. SMITH

The constructive discussion by Mr. Tuthill is appreciated particularly since he touched on several items which will be further clarified by the additional information.

There is no question but that results of most of the differences in specifications and construction which existed between the United States, represented by the Corps of Engineers, and Canada will not be apparent for a long time, if ever. It might be expected, however, that the difference in height of lift, i.e., no restriction by Canada and the 5 ft limitation by the Corps, also the delay of 120 hr before placing succeeding lifts, with no such restriction by Canada, may have affected the number of cracks now existing in the structures constructed by the two agencies.

The undesirable preponderance of material on one screen which may have been possible with the fine aggregate gradations specified by both countries is recognized. No such difficulty has been experienced, however, nor is it likely to occur in the day-to-day run from the same plant, particularly when manufactured sand is being produced. The Corps of Engineers require a gradation similar to the one proposed by Mr. Tuthill on large projects requiring the placement of low cement factor concrete. Such low cement factors were not anticipated on the Corps Seaway construction and the less restrictive gradation permitted in the Corps' "Guide Specification" was used.

Mr. Tuthill's comment that the quality of the work by both agencies was such that the concrete will be in excellent condition for a very long time is appreciated. It would be interesting to inspect the structures 100 years from now to determine whether the more conservative approach by the Corps of Engineers to curing, winter protection, height of lift, etc., had, in fact, made any difference in the durability of the concrete.

CLOSURE by GORDON M. KIDD

The authors are deeply indebted to Mr. Tuthill for his fine criticism of the subject paper. It is believed that only through the exercise of such a medium can the real benefit of a treatise be reached.

Certainly, one can only agree with Mr. Tuthill that "the resulting concrete work on both sides will be in generally excellent condition for a very long time." Moreover, Mr. Tuthill's reference to specifications and procedures that produce maximum freedom of choice of method is very apt. We believe that this should be the aim of all designers and specification writers.

It has been found in some locations on the continent that the practice of procuring and supplying aggregates to the contractor is not advisable. No doubt cost analysis could prove that engineering and inspection costs are increased. In this case, the authors feel that the methods used by both agencies were the best for that particular job. It must be remembered that the inception, design, and construction of the locks was completed in a little over 2 years. That is, while construction contracts were being prepared the sources of material were being sought and contracts for the manufacture of aggregates were let even before the construction contracts. This situation was a great help in expediting the work. Inspection of the aggregate was minimized, possibly due to the excellent sources, but also by dovetailing the work of the inspection parties, first at the quarries and later at the construction site. This was particularly exemplified in one of the Canadian subcontracts for a wharf when the contractor supplied his own aggregate from his own quarry. An inspection staff four times larger than that required at any of the locks was necessary to maintain control. On the United States side, the New York Power Authority initiated the contract for the supply of aggregates which was later to be taken over by the concrete contractor. This did not prove successful and it is reported that the Power Authority was forced to purchase the aggregate for the power house contractor. This is an example where the aggregate contractor supplying directly to the concrete contractor method failed to work due to inspection and delivery problems.

Again Mr. Tuthill's point regarding the common error in specifying fine aggregate grading limitations is well taken. The percentage retained method, although possibly preferable in some cases, does not completely rule out the percent passing method. Although it is apparent that large gap graded aggregates may meet the specifications, sands from natural deposits seldom conform to this gap grading. The usual standard specification was therefore adopted for the Seaway work.

Commenting on the placing practices, no doubt the preparation of horizontal construction joints by the wet sand blasting method is best; however, for large mass structures where the shear value of a construc-

tion joint is considered negligible, the method of cleanup becomes commensurate with the requirements and working conditions at hand. Here again "maximum freedom of choice of method" becomes a factor requiring a fair degree of latitude in the specifications.

Some exception must be taken to Mr. Tuthill's remarks with regard to the transportation of concrete on a chute or belt. It is not the intention of any designer of equipment for concrete to try to eliminate segregation during transportation. Since any concrete tends to segregate even when discharging from a mixer and since it is possible to remingle all materials into a homogeneous mass before final disposition, then the method of transportation must be limited only to keeping the materials in close proximity for remingling but not allowing any spilling or loss of any of the ingredients. A more recent prerequisite for such equipment is to minimize the loss of entrained air. Thus it is felt that various types of equipment may be used for transporting concrete without unduly limiting the contractor to specific pieces and sizes provided he takes the necessary steps to produce homogeneous concrete of the desired quality and air content in the form.

"The casual Canadian approach to temperature control of mass concrete" as referred to by Mr. Tuthill is actually far from casual. The ASCE paper "High-Lift Construction Methods for Mass Concrete,"* by Otto Holden, points out that considerable study has been carried out by the Hydro-Electric Power Commission of Ontario on this subject. This paper reveals that the temperature stresses developed in high-lift construction are less damaging than those in low-lift. Furthermore, to simulate the stress conditions of the high-lift method when low-lift construction is used, the superimposed lifts should follow at close intervals which is a direct contradiction to the American practice. Therefore, we would agree that the choice of the high-lift or low-lift method should be left with the contractor provided the sequence and timing of successive monoliths required by each method is stipulated.

What the maximum size aggregate in concrete mixes should be has always been debated, and is usually resolved by selecting that size most easily produced. No doubt the heat of hydration is an important reason for selecting the largest size aggregate possible, although the smaller size (i.e., 3 in. maximum) allows for greater variety in plant and equipment (placing) which, as it was pointed out in the discussion, is extremely desirable in all contracts. It would also appear that the smaller sizing of aggregate was well selected on the basis of attaining high strength concrete as indicated in the recent paper by Stanton

*Holden, Otto, "High-Lift Construction Methods for Mass Concrete," *Proceedings*, ASCE, V. 85, PO 3, June 1959, Paper No. 2061.

Walker and Delmar L. Bloem, "Effect of Maximum Size of Coarse Aggregates on Properties of Concrete," issued by the National Ready Mixed Concrete Association.

Referring to the query regarding difference in the amount of water used in apparently similar mixes, it should be remembered that in the Canadian mix 10 percent more sand was required, whereas in the American mix a blended sand was used. No doubt the different sand content and particle shape, in addition to the different types of cement used, would account for some of the difference in water requirements.

A final point which was not clarified in the paper but for the record should be set right now is that the American method of placing concrete required layers or lifts of about 20 in. whereas the Canadian restriction was 12 in. minimum.

It is only with the greatest respect and appreciation of the discussion paper that the aforementioned points have been made. Possibly in some way these remarks may stimulate thought and controversy so that the problems of such a project as reported in the paper may be fully appreciated and thus future projects may benefit, even if only by promoting the writing of more concise, comprehensive, and practicable specifications.

Discussion of a paper by James R. Libby:

Critique of Current Methods of Varying Prestressing Moment in Pretensioned Prismatic Beams*

By M. R. MONTGOMERY†

Mr. Libby has brought to the attention of the concrete industry a point which should receive considerable thought and investigation — the economy of using bond breaking as a method of varying the prestressing moment is evident, and has been successfully used by our firm for a number of years.

Originally, the use of smooth wire and Dorland clip positive anchorages at various points within the beams and girders plus oiling of the smooth wire between the ends of the girders and the selected points of anchorages was developed by Robert Dorland.

With the development of seven-wire strand, numerous tests were run with paper and tape bond breaking. Later, split plastic tubes were used and proved more economical. Presently, we are using a commercial retardant on the portions of the strands where we desire to prevent bond, since these retardants can be controlled to prevent setting of the concrete around the strands until release time has been reached. We agree with Mr. Libby that closer inspection is required to assure accuracy of coating. We use a material with definite color and have had no trouble.

We believe the retardant has a definite advantage in that the concrete around the strands will be very close to the strand in final location and does not introduce any foreign material. Since bridge girders and other types of prestress work exposed to the elements are normally enclosed in cast-in-place diaphragms, and since good practice requires the sealing of the ends of all exposed strand or posttensioned work, we can see no reason for the Bureau of Public Roads stand in this matter. It is hoped that this paper will bring out the reasoning behind such a position.

As a practical demonstration of the effective action of bond prevention and a means of visual inspection to assure that the pattern of strand action is as the design called for, Fig. A shows the position of the strands after release of the pretensioning force on the bed.

*ACI JOURNAL, V. 31, No. 5, Nov. 1959 (*Proceedings* V. 56), p. 391. Disc. 56-26 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 31, No. 12, June 1960 (*Proceedings* V. 56).

†Member American Concrete Institute, General Manager, San Diego Prestressed Concrete, San Diego, Calif.

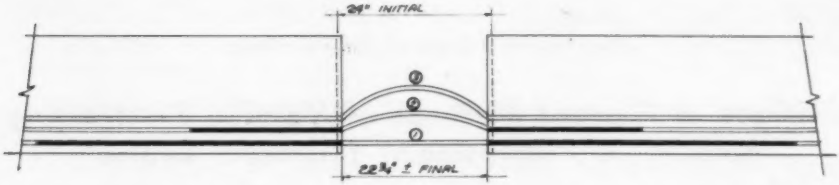

Fig. A—Assume strand "1" with bond broken for 8 ft in girder and a total of 18 ft thus free to contract, and neglecting restraining force of the dead weight of the girders, the two ends should move together approximately 18 × 0.078 or 1.4 in. Strand "2", with only 4 ft unbonded or a total of 10 ft, will contract only about ¾ in. with the rest of the length absorbed in buckling of the strand. Strand "3" can only absorb about 0.15 in. in contraction, therefore will have a larger bend. This visual evidence, like so many other features of pretensioned prestressed concrete members, provides immediate and positive evidence of any slip-up in the manufacturing process

Each girder in the string will move toward its neighbor an amount equal to the elongation of the longest unbonded strand, less the frictional resistance to sliding of the beam on the soffit. The strand with the greatest unbonded length will be straight and have some tension remaining, with the other strands bending in proportion to their unbonded length (see Fig. A).

We agree most heartily with Mr. Libby that additional systematic research is needed in this field, as in all other fields of prestress work, and believe that, with the increased cost of both labor and materials, such work is definitely justified by both manufacturers of prestressed concrete and the manufacturers of cement and prestressing steel to keep concrete in its best economic position in the tough competition ahead of the industry.

Discussion of a paper by A. C. Scordelis, T. Y. Lin, and R. Itaya:

Behavior of a Continuous Slab Prestressed in Two Directions*

By A. H. BROWNFIELD and AUTHORS

By A. H. BROWNFIELD†

The writer and several others have been intimately connected with the testing program since its inception during 1955. The need for tests on prestressed flat slabs became apparent when designs and plans were first submitted for approval to the Division of Architecture. Field observations and design calculations confirmed this need, so a limited testing program was then authorized. The use of small lucite models was rejected because no ready method of simulating the effect of the strands appeared feasible. Therefore, a concrete model was necessary and the race against time and limited funds began so as to glean as much information as possible. The model was made as large as practicable within the laboratory. It served the intended purpose well and the results were satisfactory. The Engineering Materials Laboratory at the University of California is in continuous service and a large model like this one cannot be held for future study.

When using the experimental values of 75-25 percent of total moment, the question arises, should we adopt the upper limit suggested for the column strip since the differential moment is large? While this is possible, several problems are created, such as high edge stresses, and possibly differential long-time creep and shrinkage within the slab. If the strands are spaced to accomplish this distribution, what should the stressing sequence be to prevent cracks? Do the direct concrete stresses become constant and uniform at some finite distance from the ends of the strand anchors? These questions are suggested, not necessarily for direct answers, but to create thought by engineers who may wish to use the design data presented in this paper. For example, in a waffle slab it may be prudent to lower some of the strands, that is, reduce the sag in the middle strip and use a nearly constant strand force across the entire panel. The generous use of mild steel for waffle slabs is recommended in all bottom flanges where there is tension or a reasonable

*ACI Journal, V. 31, No. 6, Dec. 1959 (*Proceedings* V. 56), p. 441. Disc. 56-28 is a part of copyrighted Journal of the American Concrete Institute, V. 31, No. 12, June 1960 (*Proceedings* V. 56).

†Member American Concrete Institute, Supervising Structural Engineer, California Division of Architecture, Sacramento, Calif.

possibility of tension due to loading, shrinkage, creep, and temperature changes (see p. 33 et seq. of Reference 6).

REFERENCE

6. Lin, T. Y.; Scordelis, A. C.; and May, R. H., "Behavior of Prestressed Concrete Beams at Transfer," *Structures and Materials Research Series*, Division of Civil Engineering, University of California (available from State of California, Printing Division, Documents Section, Sacramento 14, Calif.)

AUTHORS' CLOSURE

Mr. Brownfield's discussion was most welcome since he took such an active part in the development of the project described. In addition he has been one of the pioneers in the use of prestressed lift slabs in California.

The authors agree with Mr. Brownfield that a 75-25 percent distribution of prestress force between the column and middle strip should not be used in design. First of all, this is only the ratio of column to middle strip moments obtained in the test at the interior line of supports. At midspan the ratio was 60-40 percent. In addition a 75-25 percent distribution of prestressing force does not necessarily produce a 75-25 percent distribution of moments. This can be seen in Fig. 9 where the moments produced by a 1.8-1 prestress ratio are shown.

It is the authors' belief that a compromise value of 60-40 or 65-35 would work much better from the point of view of more uniform compression in the concrete and less concentration of high prestress.

Mr. Brownfield's suggestion of reducing the sag in some of the cables may be a good solution at certain times, especially if a minimum amount of compression is to be provided to prevent cracks in the slab. The disadvantage of reducing the sag is that the ultimate moment capacity is reduced.

A properly designed lift slab should have minimum camber and deflection under its most probable long-time working load and it should have sufficient over-load capicity as determined by ultimate strength. These two criteria can best be satisfied in design by using a combination of prestressing steel and reinforcing steel. This approach to the design of prestressed lift slabs has been described in a recent ACI paper by Rice and Kulka.[7]

REFERENCE

7. Rice, Edward K., and Kulka, Felix, "Design of Prestressed Lift Slabs for Deflection Control," ACI JOURNAL, V. 31, No. 8, 1960 (*Proceedings* V. 56), pp. 681-694.

Discussion of a paper by William L. Zemaitis:

Factors Affecting Performance of Unit-Masonry Mortar*

By JOHN W. McBURNEY and AUTHOR

By JOHN W. McBURNEY†

The author is commended for having added another paper to the relatively small number in which direct comparisons are reported between the properties of mortar made with masonry cement and with portland cement-lime mixtures. Before discussing Mr. Zemaitis' paper, a brief historical review of mortar and mortar materials may be in order.

The Mesopotamian, Egyptian, and Greek civilizations made little use of mortar materials as known today. Bitumen appears to have been used as a jointing materials in some Babylonian masonry. Gypsum has been identified in some of the Egyptian pyramids. Stone in Egyptian and Greek monumental structures was usually laid without mortar. The earliest surviving description of mortar and mortar materials is by the Roman architect Vitruvius.[14] This was written not later than 14 A.D., being dedicated to the first Roman Emperor, Augustus Caesar. This treatise was principally on architecture but described and discussed limes, hydraulic limes, natural cements, pozzolans, and sands. Celebrated architects of the Renaissance, such as Michelangelo, considered Vitruvius as their authority. Various individuals starting about the middle of the 18th Century investigated the differences in the behavior of pure limes and hydraulic limes and sought the reason for these differences. It was noted that firing limestone with clay gave hydraulic properties to the powder obtained by grinding the resulting material. Aspdin got his patent in 1824. Vicat[15] in 1828 summarized the preceding work. The first government publication on mortars and mortar materials was by Gillmore[16] in 1863. As a matter of interest, the first importation of portland cement was in 1865 and production started in the United States in 1876. During the period of masonry fort building by the Army Engineers (1820-1845), the recommended mortar was 1:3 lime-sand, with Rosendale natural cement used for pointing. The preferred lime was a very plastic high-calcium lime from Thomaston, Me. Among the forts in which

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Thomaston lime was used were Fort Pulaski in Georgia and the stone fort in Boston harbor.

After the introduction of portland cement there was a period in which 1:3 natural cement-sand mortars were replaced by 1:3 portland cement-sand. The first model code of the National Board of Fire Underwriters,[17] issued in 1905, permitted three mortars (1:3 portland cement-sand, 1:1:6 portland cement-lime-sand, and 1:3 lime-sand, all by volume). The 4th edition (1920) of the same code specified the same three mortars but replaced the 1:3 portland cement-sand by a 1:¼:3 cement-lime-sand. The next step was the reappearance of 1:3 natural cement-sand mortars as the permissible equivalent of the 1:1:6 cement-lime-sand mix.[18] The high portland cement mortars were obviously in disfavor because of their harsh working and the common occurrence of leaky masonry resulting both from poor bond with units and defective workmanship associated with low plasticity. Apparently, the first official use of the term "masonry cement" was U. S. Government Master Specification No. 443 issued in 1926. This was superceded by Federal Specification SS-C-181 in 1931 and its revisions (a), (b), and (c) following in 1936, 1938, and 1954.

Palmer and Parsons[19,20] presented two important papers in 1934 which compared the properties of cement-lime with masonry cement mortars. What are essentially modern laboratory methods were used in this research. The mortar materials included two brands of portland cement, five brands of lime, and 13 masonry cements. Six different types of brick were used where mortars were tested in assemblages. One important conclusion was that the properties of brick and mortar must be considered together in the evaluation of mortars. The second paper[20] reported the permeability to water of brick-mortar assemblages.

The Rogers and Blaine paper[21] was also published in 1934. This was confined to the properties of masonry cements and included 41 brands. No brick-mortar assemblages were tested. The masonry cements included hydraulic limes, natural cements, blast furnace slag cements containing various additives, or portland cement with and without additives. An attempt was made to identify the additives. About half of the 41 brands contained additions referred to as "water repellent." Plasticizer is a more recent name. It is suspected that some of these additives (tallow, for example) would now be considered as air-entraining agents. The authors commented, "The majority of these masonry cements have been on the market for this purpose but a relatively short time."

The uncertainty of these 1934 papers is the relation of the masonry cements then sampled and tested to today's materials. The realization that the composition and properties of a given brand of masonry cement may change or even that the brand may be withdrawn from the market before the investigation is finished and the results published has kept

some of us out of this field of research. Brand names themselves are rather permanent.

The Watstein and Seese 1947 paper[22] compared 1:¼:3, 1:1:6, and 1:2:9 cement-lime-sand mortars made with one brand of portland cement and five brands of hydrated lime with four 1:3 mortars made with masonry cement. One brand of brick was used in the brick-mortar assemblages tested for strength of bond in tension. The bricks were sorted and selected so that the initial rate of absorption at 1 min was not less than 5 or more than 15 g. Air entrainment was not measured but the probabilities are that some, if not all, of the masonry cements had air-entraining additives. The usual specification tests were reported and also the bond strength of brick-mortar assemblages in tension. The tension tests indicated that the masonry cement (Type II) mortar had the same bond strength as the 1:2:9 cement-lime-sand mortars instead of being as strong as the 1:1:6 mortars as assumed in ASTM C 270. The average compressive strengths of the 1:1:6 and 1:2:9 mortars were 1870 and 718 psi, respectively, whereas the masonry cement mortars averaged 1146 psi.

Evans and his co-workers[11] reported the properties of 17 brands of masonry cement. At least two of these cements were not portland cement base types. This is the first paper on masonry cement known to me which gave measurements of air entrainment. "The three cements that failed in the freezing and thawing test all had air entrainment values of less than 8.0 percent. The freezing and thawing test in this investigation shows a relation between entrained air and resistance to freezing and thawing but does not appear to show a good relation between rate of absorption or weight loss and resistance to freezing and thawing. Probably a better relation of the tests would have been shown, had the freezing and thawing test been carried farther. The freezing and thawing test is a rather severe shock test and many not have the same effect on mortar prisms cast in nonabsorbent molds that natural weathering has on mortar placed between bricks."

Minnick's[23] 1959 paper compared 1:1:6 and 1:2:9 portland cement-lime-sand mortars with three 1:3 masonry cement-sand mortars. One of these masonry cements was a portland cement-hydrated lime mix. The other two were probably the usual 50-50 mixture of cement and limestone with added admixtures of an air-entraining nature. Minnick tested brick-mortar assemblages using five types of bricks from 887 to 9047 psi in compressive strength and from 19.1 to 4.4 percent absorption by weight when tested after 24 hr cold submersion. He also reported values for modulus of elasticity and initial rate of absorption of his brick. His tests are indicated by the following conclusions: "(1) The use of lime in mortar reduces the tendency for the mortar joint to leak. (2) The high lime mortars substantially reduce the efflorescence of the mortar-

brick assemblages. (3) The presence of lime in the mortar increases the bond between brick and mortar and forms well integrated structural assemblages with relatively high values of Young's modulus of elasticity, good flexural strength, and a tendency to break under load within the mortar itself rather than at the bond. In contradistinction to this, the assemblages made with lime-free mortars tend to break at the interface between the mortar and brick, generally demonstrating a lower over-all flexural strength. (4) It is indicated that repeated wetting and drying conditions are favorable to improving the water tightness of a high lime mortar brick assemblage." With reference to conclusion (3) the words of Vicat[15] seem pertinent: "If the cohesion of a mortar be much less than its adhesion, all the excess adherance will be superfluous, for the rupture or separation will then take place in the mortar." As a matter of interest, some of the breaks reported by Minnick took place in his Brick D which was the weakest of the five kinds of brick used in his assemblages. The same behavior was noted in tests at the National Bureau of Standards and reported on p. 759 of *Research Paper* RP 504 in 1932.

The papers by Messrs. Minnick and Zemaitis are not necessarily in conflict for the reason that the two authors investigated different properties. Methods also differed. Minnick tested brick mortar assemblages—Zemaitis tested mortar prisms cast in nonabsorbent molds. The differences between volumetric changes in mortars cast in nonabsorbent molds and in contact with brick was noted by Davis and Troxell[24] in 1929. The water-cement ratio would also be affected by such differences in molding.

Some questions and comments on Mr. Zemaitis' paper follow.

Should not the reference to McBurney's paper in the third paragraph be to his 1952 ASTM paper[25] instead of to the paper cited in the author's Reference 6? As a matter of interest the mortar which caused the trouble in the Iowa parochial school was made from a masonry cement which was among the 41 brands sampled by Rogers and Blaine.[21] Mr. Zemaitis classified his eight limes into three S and five N types. Since S type lime is defined in ASTM C 207-49 (hydrated lime for structural purposes) as a lime which does not contain more than 8.0 percent of unhydrated magnesia, has an Emley plasticity of not less than 200 on putty when tested within 30 min after mixing with water, and has a water retentivity not less than 85 percent when tested according to ASTM C 110-58, may not some of the N actually be S type? The S classification is not restricted to autoclaved limes. A number of high calcium limes meet S requirements. Were they tested? The absence of values for water retentivity is noted in his paper.

The doubts expressed by Evans[11] with respect to his freezing and thawing test express my views on the author's use of C 291. I am reminded of the report made by a university laboratory many years ago

which recommended the rejection of a shipment of sand-lime brick intended for building a wall because they did not pass the rattler test prescribed in ASTM C 7-42 for paving brick.

The author's Reference 6 is to a report on a 20-year exposure test of brick piers laid with cement-lime mortars conforming to the proportions of Types M, N, O, and K of ASTM C 270 with the exception that K was a 1:3 lime-sand mortar with no added portland cement. (These mortars are referred to as A,B,C, and D in the paper.) This exposure test provided the following conclusions:

(1) Where the piers were protected so that rain did not enter the top (horizontal) surface and the base was protected by a flashing, no difference in weathering as rated by visual inspection was noted after 20 years of exposure.

(2) In the absence of protection, the 1:¼:3 and the 1:1:6 mortars showed no effects of weathering.

(3) Most of the damage to the 1:2:9 and 1:3 lime-sand mortars where unprotected, took place during the first 3 years of exposure. Weather Bureau records gave an average of 50 effective freezes and thaws per year or a total of 1000 cycles. The discrepancy between the results of this exposure test and the test used by Mr. Zemaitis appears obvious.

It is rather well known that disintegration of mortar can result from other causes than freezing and thawing in the presence of moisture. As an example an old masory fort (completed in 1845) was recently examined. The location is such that the temperature of the air rarely falls below freezing. The masonry showed disintegration of the lime mortar in places as much as ½ in. Repointing with modern mortars, including several made with air-entraining portland cement-base types, failed (disappeared) within 2 years.

It should be emphasized that the behavior of mortar is much affected by the properties of the units with which it is in contact. There are also some important properties of mortar other than resistance to weathering. Pearson's paper[26] is recommended reading. My own preference is for specifications written in terms of properties rather than composition. ASTM C 91 and C 270 are moving in the right direction. The real problem is to get architects, engineers, and contractors acquainted with these specifications.

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AUTHOR'S CLOSURE

Mr. McBurney added to the limited literature on mortar by his coverage of the history of mortar.

Regarding his comments on the various papers presented in the past, it should be noted that masonry cements have changed in composition and in physical characteristics considerably in the last 3 years. Most manufacturers have improved the water retention of their masonry cements until they approach the neighborhood of 85 to 90 percent. While tests were not run on the water retention of the original two masonry cements used in this research, it is known that one of them had a water retention in excess of 90 percent. Water retention characteristics were not of particular interest in this specialized investigation. The ability of a mortar to retain its workability without retempering is of much greater interest but there is no recognized test for this important characteristic.

Mr. McBurney's remarks on bond strength are of interest, but as was pointed out in this paper, bond strength was not investigated. There is considerable evidence in some unpublished preliminary work that the bond strength of a mortar has a direct relationship to the amount of air that is entrained in the mortar. The importance of bond strength in exposed mortar is secondary if the mortar should fail in durability because, obviously, durability failure will destroy whatever bond strength is in the mortar itself.

The reference to Mr. McBurney's paper in the 1956 ASTM *Proceedings* was intended but it was also intended to use the 1952 reference as listed

by Mr. McBurney. The author regrets failing to have included the 1952 reference.

Some of the limes listed as Type N and used in this investigation may have met the Type S classification. However, the bags listed them as Type N and they were so classified. Only limited tests were run on the cements, masonry cements, and limes.

Mr. McBurney points out that freezing and thawing tests were run on prisms instead of mortar between masonry units. It is agreed that such freezing and thawing tests are severe and were intended to be severe to get data within a reasonable length of time. But, again, it must be pointed out that the air protected mortars withstood this test. It should not be assumed that because a certain low air mortar mix lasted only two cycles with about 50 percent reduction of Young's modulus that it would only take two cycles of exposure when used with masonry in the field. Obviously, the moisture condition of the wall is an important factor. It is reasonable to suppose that such a mortar would be subject to deterioration in some longer period of time, as for example, ten cycles of freezing when the wall was moist. The point is that the tests are merely relative and should be examined in that light.

There is a considerable amount of evidence of freezing and thawing damage in low air mortar mixes in masonry work where walls can become saturated with water. Unfortunately, design, materials, and workmanship have not reached the point where walls can be guaranteed to be watertight. Mr. McBurney points to a case where brick piers were exposed to weather and went through several winters of freezing and thawing. Obviously, if the design is such that water cannot get into the wall, or if the wall itself cannot become saturated, freezing and thawing damage cannot occur. It is the author's opinion that if most walls were not water resistant practically all low air mortar walls in freezing climates would have failed within a few years of the time they were built. There is no discrepancy between this paper and the exposure tests run by the National Bureau of Standards because freezing on a non-water-saturated wall should show no freeze-thaw damage. This might be compared to a non-air-entrained concrete floor inside a building which fails to show freeze-thaw damage even though the building goes through periods without heat.

This paper does not claim that disintegration of all mortar joints are due to freezing and thawing damage. Just as in concrete, other factors can cause disintegration.

In conclusion, it should be pointed out that many major northern cities have several pages of tuck pointing contractors listed in the classified section of the telephone book, indicating a considerable business in this field. A perfect example of freeze-thaw damage in mortar joints was observed in Chicago at a recent ASTM meeting. This wall showed a con-

siderable amount of efflorescence and a number of damp spots indicating that the wall was subjected to moisture from some source. The joints were badly eroded.

The author would very much like to see research done on panels subjected to freezing and thawing tests with efflorescence being allowed to form during a warming and drying cycle. It is possible that efflorescence might be excessive on those brick panels which are saturated and subjected to freezing and thawing as compared to those panels which are saturated and not subjected to freezing and thawing. An important side effect would be noted in such an investigation as to whether or not mortars unprotected by air would disintegrate almost as fast as they did in the bar tests.

The author agrees with Mr. McBurney's preference for a mortar specification written in the terms of properties rather than in the terms of composition. However, the author feels that additional properties should be tested such as the soundness of the cementitious materials in the mortar and the air content of the field mortar itself. The author also feels that a new test should be designed to test the ability of the mortar to retain its plasticity for some stated period of time. It has been proved by both the National Bureau of Standards and the Portland Cement Association that the plasticity of the mortar at the time of laying has a marked effect on the bond strength of the mortar.

Discussion of a paper by Howard L. Furr:

Numerical Method for Approximate Analysis of Building Slabs*

By WEN F. CHANG, A. C. SCORDELIS, and AUTHOR

By WEN F. CHANG†

Working on his master's thesis and some unpublished research work under Prof. Phil M. Ferguson at the University of Texas in 1952, the writer did some analysis on the subject discussed in the article. The grid analysis as presented in the article will yield reasonably accurate approximation if the strips are not too wide. The statical ratio does afford a convenient guide for the first few tries. As the deviation of the static ratio becomes smaller after each correction, adjustment of deflection will not be as apparent as the first few tries. If the joints are very numerous, this method becomes a tedious process.

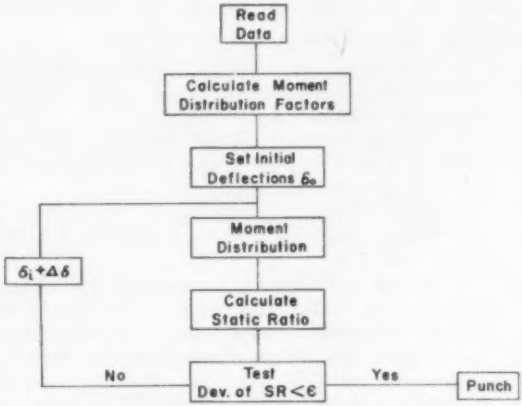

Fig. A—Computer program flow chart

The author thinks that the grid analysis can be solved by an electronic computer. Much computation time can be saved and a more accurate solution can be obtained. A flow chart, as seen in Fig. A, can be used to set up the program.

By A. C. SCORDELIS‡

A mathematical solution of the Lagrangian differential equation for plate deflections is often impractical for the analysis of slabs with cutouts or complex boundary conditions. In cases of this type the analysis of an equivalent gridwork, as indicated by the author, is an excellent method for determining approximate values of internal forces and moments, reactions, and vertical deflections. The grid method is versatile

*ACI Journal, V. 31, No. 6, Dec. 1959 (*Proceedings* V. 56), p. 511. Disc. 56-33 is a part of copyrighted Journal of the American Concrete Institute, V. 31, No. 12, June 1960 (*Proceedings* V. 56).

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in that it can be applied to a slab with any configuration, with or without openings, with constant or variable depth. The slab may be subjected to any load pattern. The method is approximate in that it replaces the continuous media of the slab by an intersecting gridwork of beams. The accuracy of the solution is dependent on the fineness of the grid used. The chief disadvantage of the method is the tremendous amount of numerical work required for a given problem. This disadvantage can be overcome to a great extent by developing a general program for this method utilizing high speed digital computers.

The analysis of the gridwork by the statics ratio method, presented by the author, is similar to the method suggested by the writer* in a discussion of the paper by Ewell, Okubo, and Abrams published in 1952.

Two alternate procedures may be used to analyze a gridwork subjected to loads at its joints. The first procedure advocated by Ewell, Okubo, and Abrams was as follows:

1. Introduce a unit vertical displacement at a single joint, with all joints locked against rotation, and all other joints prevented from displacing. Calculate fixed end moments.
2. Without allowing further vertical displacement at any joint, permit the joints to rotate and carry out a moment and torque distribution. Tabulate the moments and torques for this case.
3. Using the above moments and torques and equations of statics, compute the holding forces at each joint necessary to hold the gridwork in this deflected position.
4. Repeat the above procedure for each joint which is free to deflect in the given structure. Concepts of symmetry can be used to decrease the number of joints to be treated in this manner.
5. A set of simultaneous equations may then be written:

$$k_{11}\Delta_1 + k_{12}\Delta_2 + k_{13}\Delta_3 + \cdots + k_{1n}\Delta_n = P_1 \quad (1)$$
$$k_{21}\Delta_1 + k_{22}\Delta_2 + k_{23}\Delta_3 + \cdots + k_{2n}\Delta_n = P_2 \quad (2)$$
$$k_{31}\Delta_1 + k_{32}\Delta_2 + k_{33}\Delta_3 + \cdots + k_{3n}\Delta_n = P_3 \quad (3)$$
$$\cdots \quad \cdots \quad \cdots \quad \cdots \quad \cdots$$
$$\cdots \quad \cdots \quad \cdots \quad \cdots \quad \cdots$$
$$k_{n1}\Delta_1 + k_{n2}\Delta_2 + k_{n3}\Delta_3 + \cdots + k_{nn}\Delta_n = P_n \quad (n)$$

In which:

$k_{11}, k_{21}, k_{31}, k_{n1}$ are the holding forces developed at joints 1, 2, 3, . . . n due to $\Delta_1 = 1$

$k_{12}, k_{22}, k_{32}, k_{n2}$ are the holding forces developed at joints 1, 2, 3, . . . n due to $\Delta_2 = 1$

$P_1, P_2, P_3 \ldots P_n$ are the actual joint loads for which $\Delta_1, \Delta_2, \Delta_3, \ldots \Delta_n$ are desired

n is the number of joints free to deflect in the given structure. The above equations may be conveniently expressed in matrix form as follows:

$$\underset{n \times n}{[K]} \quad \underset{n \times 1}{\{\Delta\}} = \underset{n \times 1}{\{P\}}$$

The $[K]$ matrix is commonly known as the stiffness matrix.

6. The simultaneous equations are then solved for $\Delta_1, \Delta_2, \Delta_3, \cdots \Delta_n$, after which the final moments and torques may be obtained by multiplying

*Discussion by A. C. Scordelis of "Deflections in Gridworks and Slabs," by W. W. Ewell, S. Okubo, and J. I. Abrams, *Transactions*, ASCE, V. 117, 1952, pp. 892-897.

values obtained in Step 2 by the appropriate value of Δ and then superposing to get the total values. Using matrix notation the deflections may be conveniently expressed in terms of the applied joint loads as follows:

$$\underset{n\times 1}{\{\Delta\}} = \underset{n\times n}{[K]^{-1}}\ \underset{n\times 1}{\{P\}} = \underset{n\times n}{[F]}\ \underset{n\times 1}{\{P\}}$$

The $[F]$ matrix, commonly known as the flexibility matrix, equals the inverse of the stiffness matrix, $[K]$. It should be noted that the stiffness and flexibility matrices are functions solely of the geometry and properties of the structure and are independent of the loading. Thus they may be used with any loading condition.

The second procedure, advocated by the author, replaces the solution of a series of simultaneous equations for the deflections by a method of successive corrections in which the deflections are adjusted until the statics ratio is the same for all shear equations. The number of independent shear equations in this method must be equal to the number of possible joint deflections.

The second procedure has both advantages and disadvantages when compared with the first procedure. Some of the advantages are:

1. A single solution is required no matter how many joints are involved.
2. No simultaneous equations have to be solved.
3. Final answers for moments, torques, and deflections are obtained at the end of a single solution.
4. The analyzer can use his judgment and experience in estimating the relationships between various joint displacements, thus speeding the solution.
5. The method of first using a coarse grid, and then with the displacements obtained therefrom proceeding to a finer grid, may be pursued.

Some of the disadvantages are:

1. In certain cases, involving many joints, the writer has experienced considerable difficulty in obtaining convergence. A deflection introduced at one joint affects several shear equations making it difficult to obtain the same statics ratio. It would be of interest to learn of the author's experience in this regard.
2. The single solution necessary using the second procedure is much longer and more complex than each of the independent joint displacement solutions required by the first procedure.
3. Each new loading condition involves a separate solution whereas by the first procedure much of the work need not be repeated for successive load conditions.

One of the chief objections to the first procedure at the time Ewell, Okubo, and Abrams published their paper in 1952 was the necessity of solving a large number of simultaneous equations. Since that time there has been a rapid development and increase in the use of high speed digital computers so that this is no longer a serious objection. Standard programs are available on most computers for the solution of simultaneous equations or matrix inversions.

In any particular problem the analyzer must decide which of the two procedures described above possesses the greater advantages. In any event he should be familiar with both approaches so that he may make the correct decision.

AUTHOR'S CLOSURE

Mr. Chang is acquainted with this method having made the analysis for the flat slab in Fig. 9. Fairly good approximate results can be obtained by using wide strips, but, as he points out, narrow strips are desired for greater accuracy. The first few tries at balancing are generally all that are required. Some deviation from an exact balance must be permitted, and it is a waste of time to try to reach an exact balance throughout the net by use of the pencil and paper method.

Both Professor Scordelis and Mr. Chang point out the desirability of a computer solution, and the author agrees with them. It is felt that the method outlined by Martin and Hernandez* will possibly require a simpler computer program than would be necessary with the statics ratio method. Martin's and Hernandez' method is being explored with the view of applying it to a computer solution.

The difference between the method of successive shear corrections, outlined by Professor Scordelis, and the statics ratio method is in the procedure used to arrive at a balanced condition. The former requires that an exact balance be attained between load and shears at a joint; the statics ratio method requires that the applied load be some multiple of the joint shears, and that the multiple be the same value for each joint in the grid. The value of the multiple (statics ratio) is, in theory, immaterial because the true shear is found by dividing the computed shear by the statics ratio. The analyst is free to choose the value of the statics ratio that he wants to balance on, and he may change the value at any stage of computations to fit the situation as it exists at the time.

As Professor Scordelis points out, the solution becomes more difficult as the number of joints increases. All loadings and slabs used by the author were symmetrical, and seven equations for seven joints in a half-quadrant sufficed for a solution. No particular difficulty was met in arriving at a solution using seven equations, but the solution was more difficult in general than one using a smaller number of joints. The greatest difficulty met in balancing any grid was that of the slab shown in Fig. 7 and 8. The stiffnesses of beams framing into a joint were different in the two orthogonal directions and it was very hard to estimate even approximately the change needed in deflection to give a desired change in statics ratio. It is easier to balance more joints with beams of uniform stiffness than it is to balance fewer joints with beams varying one with the other in stiffness.

ACKNOWLEDGMENT

The material in the original paper was taken largely from the doctoral dissertation of the author in the Graduate School of the University of Texas, Austin. The work was performed in the Department of Civil Engineering under the direction of Prof. Phil M. Ferguson.

*Martin, Ignacio, and Hernandez, Jose, "Orthogonal Gridworks Loaded Normally to Their Planes," *Proceedings*, ASCE, V. 86, ST 1, Jan. 1960, pp. 1-12.